THURSDAY, DECEMBER 19, 1889.

THE EPIDEMIC OF INFLUENZA.

FOR the first time after an immunity of nearly half a century, our country is again threatened with an epidemic of influenza. The accounts we receive of epidemic illness in Russia, in Germany, and last of all in Paris, seem to make its irruption here every week more imminent. The question will, however, naturally be asked by the public, whether there is any real ground, in the history and in what is known of the nature of the disease, for such an apprehension? Is it a disease really brought from a distance? Is it anything more than the general prevalence of catarrhal affections, of colds and coughs, which the time of year, and the remarkably unsettled weather we have lately experienced, make readily explicable without any foreign importation? Indeed, is influenza, after all, anything more than a severe form of the fashionable complaint of the season?

To answer the last question first, and so to put it by, there can be little doubt that influenza is a distinct, specific affection, and not a mere modification of the common cold. The grounds for this belief cannot be fully stated here, but may be gathered by reference to the descriptions of the disease as seen in former outbreaks by physicians of the older generation; for instance, by Sir Thomas Watson in his classical "Principles of Physic," or the late Dr. Peacock in his article in Quain's "Dictionary of Medicine."

These symptoms, the history of the disease, and its distribution, all justify us in treating it as a distinct and specific disease, which when it is prevalent will rarely be mistaken, though, with regard to isolated and sporadic cases, difficulties of diagnosis may arise. About its nature, or its affinities with other diseases, it is unnecessary to speculate. It will be sufficient to inquire what its recorded history in the past justifies us in expecting as to its behaviour in the future. There are few cases in which history proves so important an element in the scientific conception of a disease as it does in that of influenza. For hardly any disease shows a more marked tendency to occur in epidemics—that is, in outbreaks strictly limited in point of time. After long intervals of inaction or apparent death, it springs up again. Its chronology is very remarkable. Though probably occurring in Europe from very early times, it first emerged as a definitely known historical epidemic in the year 1510. Since then, more than 100 general European epidemics have been recorded, besides nearly as many more limited to certain localities. Many of them have in their origin and progress exhibited the type to which that of the present year seems to conform. We need not go further back than the great epidemic of 1782, first traceable in Russia, though there believed to have been derived from Asia. In St. Petersburg, on January 2, coincidently with a remarkable rise of temperature from 35° F. below freezing to 5° above, 40,000 persons are said to have been simultaneously taken ill. Thence the disease spread over the Continent, where one-half of the inhabitants were supposed to have been affected, and reached England in May. It was a remarkable feature in this epidemic that two fleets which left Portsmouth about the same time were attacked by influenza at sea about the same day, though they had no communication with each other or with the shore.

There were many epidemics in the first half of this century; and the most important of them showed a similar course and geographical distribution. In 1830 started a formidable epidemic, the origin of which is referred to China, but which at all events by the end of the year had invaded Russia, and broke out in Petersburg in January 1831. Germany and France were overrun in the spring, and by June it had reached England. Again, two years later, in January 1833, there was an outbreak in Russia, which spread to Germany and France successively, and on April 3, the first cases of influenza were seen in our metropolis; "all London," in Watson's words, "being smitten with it on that and the following day." On this same fateful day Watson records that a ship approaching the Devonshire coast was suddenly smitten with influenza, and within half an hour forty men were ill. In 1836 another epidemic appeared in Russia; and in January 1837, Berlin and London were almost simultaneously attacked. Ten years later, in 1847, the last great epidemic raged in our own country, and was very severe in November, having been observed in Petersburg in March, and having prevailed very generally all over Europe.

Some of these epidemics are believed to have travelled still further westward, to America; but the evidence on this point seems less conclusive. Without entering on further historical details, and without speculating on the nature of the disease, we may conclude that these broad facts are enough to show that a more or less rapid extension from east to west has been the rule in most of the great European epidemics of influenza; and that therefore its successive appearance in Russia, Germany, and France, makes its extension to our own country in the highest degree probable.

There are, it is true, certain facts on the other side, but they appear much less cogent. Since our last great visitation, certain epidemics of influenza have been recorded on the Continent which have not reached our shores. One was that of Paris in 1866–67; another at Berlin in 1874–75, of a disease described by the German doctors as influenza, and of great severity, affecting all classes of society. But in all epidemic and even contagious diseases there are outbreaks which seem to be self-limited from the first, showing no tendency to spread. This has been notably the case with plague and cholera. On the other hand, when an epidemic shows an expansive and progressive character, it is impossible to predict the extent to which it may spread. And the present epidemic, it must be confessed, appears to have this expansive character.

Many interesting points are suggested by this historical retrospect. What is the meaning of the westward spread of influenza, of cholera, and other diseases? Is it a universal law? To this it must be said, that it is by no means the universal law even with influenza, which has spread through other parts of the world in every kind of direction, but it does seem to hold good for Europe, at least in the northern parts. The significance of this law,

as of the intermittent appearances of influenza, probably is that this is in Europe not an indigenous disease, but one imported from Asia. Possibly we may some day track it to its original home in the East, as the old plague and the modern cholera have been traced.

As regards, however, the European distribution of influenza, it has often been thought to depend upon the prevalence of easterly and north-easterly winds. There are many reasons for thinking that the contagium of this disease is borne through the air by winds rather than by human intercourse. One reason for thinking so is that it does not appear to travel along the lines of human communications, and, as is seen in the infection of ships at sea, is capable of making considerable leaps. The mode of transmission, too, would explain the remarkable facts noticed above of the sudden outbreak of the disease in certain places, and its attacking so many people simultaneously, which could hardly be the case if the infection had to be transmitted from one person to another.

Another important question, and one certain to be often asked, is suggested by the last—namely, whether influenza is contagious. During former epidemics great care was taken to collect the experience of the profession on this point, and its difficulty is shown by the fact that opinions were much divided. Some thought the disease could be transmitted by direct contagion, while others doubted it. But there was and is a general agreement that this is not the chief way in which the disease spreads, either in a single town, or from place to place.

We must avoid the fascinating topic of the cause of influenza, or our limits would be speedily outrun. But one simple lesson may be drawn from the facts already mentioned—namely, that the disease is not produced by any kind of weather, though that, of all possible causes of disease, is the one most often incriminated in this country. It is true that some of our worst epidemics have occurred in winter, but several have happened in summer; and the disease has been known in all parts of the world, in every variety of climate and atmospheric condition; so that it is certainly not due to a little more or less of heat or cold, moisture or dryness. Its constancy of type, the mode of its transmission, its independence of climatic and seasonal conditions, all suggest that its cause is "specific,"—that is, having the properties of growth and multiplication which belong to a living thing.

Whether the disease affects the lower animals is not absolutely certain, but the human epidemic has often been preceded or accompanied by an epidemic among horses of a very similar disease. It is pretty well known that such a disease is now very prevalent among horses in London. Nearly three weeks ago, one of the railway companies in London had 120 horses on the sick list, and the epidemic is still by no means extinguished. To a certain extent this must be taken as prognostic of human influenza.

It may be asked, if the influenza is really to come, can we form any notion how soon it is likely to appear? On such a point little beyond speculation is possible, for the rate at which the disease travels is extremely variable. Generally, it has taken some weeks, or even months, to traverse Europe, but occasionally much less, as, for instance, in 1833, when it appeared to travel from Berlin to Paris in two days. It is now barely a month since the epidemic became noticeable in Petersburg, where, according to a correspondent of the *British Medical Journal*, it began on November 15 or 17, though sporadic cases had undoubtedly occurred earlier. In the beginning of December it was already widely spread throughout Russsia, and, as it would seem from the published accounts, must have been in Berlin about the same time. In Paris the first admitted and recorded cases occurred about December 10, though doubtless there were cases before that date. Both public and private accounts report it exceedingly prevalent there now. In London, notwithstanding the abundance of colds and coughs, and the mysterious rumours which have been afloat, it appears to the present writer doubtful whether any cases of true influenza have yet occurred. But according to its apparent rate of progress, it might, if coming from Paris, have already arrived here; and it may be breaking out even while these lines are going through the press. But, on the whole, one would be disposed to give the epidemic another week or two. If its distribution depends, as it seems to do, on the winds, it is impossible to prophesy with much plausibility. A steady breeze setting in from one of the affected places might bring us an invasion in a very short time; but the current of air would have to be continuous over the whole district. Light local winds, whatever their direction, would, if the hypothesis be correct, have little effect. On the other hand, a steady frost, with an "anticyclone" period, might effectually keep off the disease. If, then, there is anything in the views above stated, prophecy belongs rather to the province of the weather-doctors than of the medical doctors.

Should the prospect seem a grave one, it may be some consolation to remember that an epidemic of influenza rarely lasts more than a few weeks—three to six—in one place; that it is rarely a fatal disease, though affecting large numbers of people; and that the present epidemic seems to have displayed on the Continent a decidedly mild type, which, according to the general rule, it is likely to retain. J. F. P.

THE HORNY SPONGES.

A Monograph of the Horny Sponges. By Robert von Lendenfeld. (London: Published for the Royal Society by Trübner and Co., Ludgate Hill, 1889.)

WITHIN the last few years, and as a direct result of the famous Expedition of the *Challenger*, three most important monographs of the sponges belonging to the groups of the Hexactinellida, Monaxonida, and the Tetractinellida have been published, nor must the valuable contributions by Polėjaeff to the history of the remaining groups, Calcarea and Keratosa, be overlooked. The Calcarea had the advantage of having been already monographed by Haeckel, and so there only remained the Horny Sponges to be fully described, in order that the natural history of the sponges should be up to date.

Such a work has now been accomplished—thanks to the liberality of the Royal Society—by the labour and scientific skill of Dr. Robert von Lendenfeld. This monograph forms a fine quarto volume of over 900 pages, with an atlas of fifty lithographed plates.

While a student at the University of Graz, Lendenfeld

tells us, his time was chiefly spent in the zoological laboratory of Prof. F. E. Schulze, then engaged on those researches on the natural history of sponges with which his name will ever be associated. This led him to take a special intest in the group, and to work out its history, first in the Mediterranean, and then at Melbourne and other places on the southern coast of Australia—a coast exceedingly rich in organisms of this class. From Melbourne, New Zealand was visited, and the Christchurch and Dunedin collections were examined. Next, that apparent El Dorado of the spongologist, Sydney, was explored, and, thanks to the splendid liberality of Sir William Macleay, Lendenfeld was enabled to establish a laboratory at the water-edge, and to study in a very thorough manner the sponges of this district.

With such abundant material, and with such ready help, nothing was wanting to work out the structural history of the species of the group. But to describe and name them, reference to type specimens was, above all things, necessary, and these latter were to be found most conveniently in the British Museum; thither, therefore, Lendenfeld came, early in 1886, at first resolved to write an account of the Australian Horny Sponges; but fortunately finding, during the progress of this work, that so great a proportion of the known forms were Australian, he determined to make a complete monograph of the group, and hence the volume which we proceed to notice.

This monograph of the Horny Sponges is divided into three parts: (1) an introduction, containing a brief historical summary and a detailed list of publications relating to sponges; (2) an analytical portion, devoted to the systematic description of all the known Horny Sponges; and (3) a synthetical part, in which the anatomy and physiology of sponges, especially of Horny Sponges, are treated, and their phylogeny, systematic position, and classification discussed.

Of the very extensive and scattered literature relating to the sponges, a most excellent bibliography is given; the papers are arranged alphabetically under their authors' names, but the publications of each author are given chronologically; the number of pages in each memoir is given, but, unfortunately, no reference is made to illustrations; abstracts and translations of papers are always quoted.

Considering the genus as "the important unit," the analytical part consists essentially of a series of monographs of the genera of Horny Sponges, but "species" as such are described; and the author has "done his best to make the different species equivalent," though this has been difficult of achievement. In those cases where he has felt compelled to establish varieties, he has followed the plan of E. Haeckel and F. E. Schulze, and has divided the whole species into "the requisite number of equivalent varieties." The total number of the species and varieties described amounts to 348, of which no less than 258 have been found in the Australian area.

It would not be possible, within any reasonable space, to give any satisfactory details of the analytical portion of this monograph. The descriptions of each genus are grouped into—an historical introduction; a sketch of the shape, size, colour, surface, and rigidity characteristic of the group; an account of the canal system, skeleton, with notes on the histology and physiology; the affinities of the genus; statistics of the species, with a key thereto, and details of distribution. Doubts must of necessity arise as to the exact limits that each author would ascribe to the species described by him, and in doubtful cases of this sort Dr. Lendenfeld has adopted the plan of placing no authors' names after them, but gives a full list of synonyms; we think it a pity that in these lists the memoirs, instead of being quoted, are simply referred to by numbers, for the explanation of which one must refer to the bibliographical list.

It is in the synthetical part, in which the general results are discussed, that the chief interest of this work lies, at least for the general reader. Here we have the questions of the general structure and evolution of sponges as a group considered, and their classification and systematic position discussed; and finally, as the fashion of some authors is, "an ancestral tree of the families" is given. Starting with the story of the metamorphic development of sponges, we find the primitive sponge defined as consisting of a simple ento- and ectoderm, and a thin mesoglœa—a very primitive mesoderm—between the two. Dr. Lendenfeld thinks that it is now generally acknowledged that the Physemaria, which Haeckel considered as "Gastreaden der Gegenwart," are not sponges at all, but Protozoa, so that they need not here be taken into account. Of course, it is evident that the views about these Physemarias, held at present by Haeckel, were, at the time of his thus writing, unknown to Dr. Lendenfeld. The modified Gastræa is traced onwards in its development, and the morphology of the adult structures are passed under review; their want of symmetry—and the exceptions are but few—is noted. None of the Horny Sponges are green; blue is never observed in the group, the range of colour being from light yellow to dark brown, light to dark red, and light to a dark, almost black, violet; the colour is lost in all, with a few exceptions, such as in *Aplysilla violacea*, when the sponge is preserved. The Horny Sponges would seem never to imitate their surroundings in colour, but it is suggested that in some cases the intense vivid colours may have the effect of frightening their enemies.

An attempt is made to account for the shape of the sponge conuli as the result of two pressure forces and to express this by formula. The biological student will scarcely be grateful for this, and is likely to be bewildered when he reads that "the conuli are hyperbolic rotatory bodies, formed by the rotating of the hyperbola,

$$y = (p \, . \, x)/(t + t \, . \, x),$$

round an axis parallel to the direction of pressure through the summit of the conulus." The canal system is described in some detail, the author not confining himself to the Horny Sponges. In contrasting this system in the Hexactinellida and the Hexaceratina, there seems some little confusion as to the comparative "tenderness" of the structures. The absence of spicules (siliceous) in the fibres is considered as the characteristic feature of the Horny Sponges, which distinguishes them from their siliceous ancestors; but in the superficial fibres of Aulena, echinating proper spicules occur; in the ground substance of several genera of Spongeliadæ, microsclera are

to be found, while in Darwinella, triaxon horny spicules abound.

Very interesting accounts are given of the connective tissue, muscle cells, and nervous system. Stewart's account of the "palpocils" is accepted; and, although Prof. Stewart's specimens are the only ones which show these organs properly, yet Lendenfeld thinks that, when groups of converging sense-cells are observed (in sections) below the continuous surface, these may be regarded as the cells of a "retracted" palpocil.

The researches of the author have thrown but little fresh light on the subject of the occurrence of the strange "filaments" in the species of the genus Hircinia; these filaments are generally more abundant in the superficial layer than in the interior of the sponge. They may be isolated, or arranged in bundles of varying thickness, in which they are parallel. Such bundles are particularly conspicuous in *H. gigantea*, where they form a pretty uniform network which pervades the whole of the sponge. The filaments are never straight: they may be continuously and simply curved, or they are undulating. The latter form of curvature is particularly frequently observed in the filaments which are joined to form large bundles. While their abundance is subject to variation, no case of a sponge with but a few isolated filaments is on record. No apparent young stages of these filaments have been seen. Schulze's researches enabled him to make no positive statement concerning them, but they at the same time demonstrated that "no cellulose is contained in them, that they have no trace of true cellular structure, and that they contain a great deal of nitrogen (9·2 per cent. of their substance), and that they are not Algæ. The resistance of the filaments in boiling alkali is against their being ordinary Fungi, while their general chemical composition indicates no relationship to the ordinary sponge skeleton." As to the very minute dumb-bell shaped structures observed by Poléjaeff, and considered by him to be young stages of the filaments, Lendenfeld thinks that this is extremely doubtful, "particularly as nobody besides Poléjaeff has seen them in *H. friabilis* or any other sponge." But is this so? for in another paragraph we read:—

"The spherical bodies which Schmidt and Poléjaeff consider as young stages of these filaments—in fact, as terminal knots, either dropped off, or on the way to produce a filament—have also been observed and carefully studied by Schulze, who considers them as monocellular Algæ, which have nothing whatever to do with the filaments."

Lendenfeld says that "no trace of filaments or 'spores' can be detected in the young embryos which are often found in specimens of Hircinia."

On the physiology of the group, this monograph throws but little light:—

"Our knowledge of the vital functions of sponges is at present exceedingly unsatisfactory. We do not even know which parts of the sponge absorb nourishment, or, in fact, what kind of food the sponges take in. We are equally ignorant concerning their respiration and secretion."

There being then no facts to serve us as guides to knowledge, the next "best thing" is to have recourse to imaginations, and our author "thinks" that "it is by no means unlikely that the sponges may exclusively absorb liquid food—that is to say, organic substances dissolved in the water which is continuously passing through their canal system. All the other organisms in which arrangements are made to insure a continuous water current—I refer to the higher plants—absorb exclusively nourishing material in solution (the absorption of gaseous food by plants does not concern us here). The existence of a traversing canal system and a continuous water current seems to me to point to the nourishing material of sponges being in solution in the sea-water. The numerous fine sieves and filter arrangements generally, and the mere fact that the water always enters through the smaller holes and is expelled through the larger, clearly shows that the sponges are not desirous that large food-particles should enter their canal system."

Even granting that the word "exclusively" should be after the word "material," we do not quite understand the comparison of the well-known facts of plant physiology as they are presented to us in the above extract, nor see how it helps us to an understanding of how the sponge adds to its protoplasm; the undoubted power possessed by some of the sponge-cells to lay down silica, lime, &c., is quite different functionally from the phenomena attending growth and development, using these terms in Herbert Spencer's sense; but once set a thinking, our author proceeds, and telling us that a "tape-worm is an animal which takes up liquid food, and which has no special digestive apparatus, and that it evidently takes up a great quantity of material from the surrounding chyle through the apparently indifferent cylindrical ectodermal epithelium cells; that the excess material and waste products are got rid of by the nephrydia," he goes on to say that he is inclined "to think that in sponges we may have a similar mode of absorption of nourishment"; but then, where are the nephrydia or their analogues? and he thinks again "that it is not impossible that the ciliated chambers may be partly analogous to the nephrydia of the Cœlomata, and that the collar-cells may, besides performing other functions, also secrete the urine." However uncertain, he adds, this hypothesis may appear, "I think there can be no doubt that there is more probability in it than in the view, held by Carter and others of the older authors, that the ciliated chambers are merely digestive apparatus." This seems a rather dreamy hypothesis, with no facts for its foundation; but it is but fair to remark that it comes at the very end of a volume which is a record of numerous and important observations.

Under the headings variability, parasitism, and symbiosis, many interesting details are given. The author thinks that certain forms of Aulena and Chalinopsilla imitate "certain siliciferous Cornacuspongiæ. These sponges have descended from those which they imitate; and, whilst they have lost the spicules in the fibres, they have retained the outer appearance of their better protected ancestors in a most striking manner." Apparently, "the primordial sponge ancestors were free-swimming, and had no skeleton. Some produced a calcareous, others a siliceous skeleton; in both the subsequent development, the formation of ciliated chambers, which the ancestors did not possess, and the fixing of the axis and rays of the spicules, were the same. The primordial Silicea had indifferent irregular spicules, from which the

triaxon and the tetraxon spicules were developed by an adaptation of the divergent development of the canal system. The primordial forms of both lived in water rich in silica, and certain forms of both lost their spicules in consequence perhaps, of rising from deeper to shallower water, where silica is more scarce. In both, some forms have lost the skeleton altogether, while others have replaced it gradually by spongin."

While acknowledging that some authors whose opinions must carry great weight, such as Balfour, Bütschli, and Sollas, consider the sponges as a separate group, equal in value to the groups Protozoa and Metazoa, Lendenfeld cannot but conclude that the sponges are, without doubt, Metazoa, and certainly Cœlentera, in the sense of being provided with a simple body cavity.

The last twenty pages of the work are devoted to a synopsis of all the known sponges, giving the classes, families, orders, and genera. In this extremely useful list there is a short analysis of the families and orders, which is based on the labours of Vosmaer, Ridley, Dendy, Sollas, Schulze, added to those of the author's own. The author ends his treatise with the statement that "Now that all the groups of sponges have been thoroughly investigated, we may consider our knowledge of their phylogenetic affinities established on a satisfactory footing" (p. 909); but it seems well to call to mind the statement with which he closes his short preface, and with which we feel the more inclined to agree, "our present knowledge of the group . . . has only just arrived at a stage corresponding to the knowledge of the higher animals of half a century ago" (p. 5).

In concluding our only too brief notice of this important work, for which all workers on the group must thank Dr. Lendenfeld, we may mention that the sponge portraits are for the most part photo-lithographs taken from the original types; though in a few cases, where no good specimens were available, the lithographic illustrations are from drawings.

THE FLORA OF SUFFOLK.

The Flora of Suffolk. By W. M. Hind, LL.D., Rector of Honington, assisted by the late Churchill Babington, D.D., F.L.S. With a Chapter on the Geology, Climate, and Meteorology of Suffolk, by Wheelton Hind, M.D., F.R.C.S. Pp. 508, with a Map. (London: Gilbert and Jackson, 1889.)

SUFFOLK is a characteristic lowland maritime English county, the flora of which, at the present day, contains absolutely no infusion of the boreal element. Its area is about 1500 square miles. The whole surface is flat, without any prominent rocks. It is underlain by chalk, which, in the north and west, lies immediately below the subsoil, but, in the south and east, is covered by Tertiary and Glacial deposits, which at Harwich have been found to reach a thickness of 1000 feet before the chalk is reached. In White's history of the county, its soils are classified into three groups: heavy lands, in which clay predominates; mixed land, common mixed soil, rich deep moulds, fen-lands, and rich marshes; and light lands, consisting of sand over chalk. To the first set belong the soils of the western two-thirds of the county, except in the extreme north and near the coast. The mixed lands are found—one portion east of the heavy lands between the Orwell and the Stour; a second in the north, between Halesworth and Yarmouth; and a third west of the heavy lands between Holston and Newmarket. The sandy, or light, soils are in the extreme north-west, in what is called the "Breck district," between Thetford and Mildenhall, where are found the rarest plants of the county, such as *Veronica hybrida*, *V. triphyllos*, *V. verna*, and *Apera interrupta*. The coast is remarkable for the extent of its tidal estuaries and bays, creeks and havens. There are no cliffs of any considerable height, but a great extent of sand and shingle. The beach at Orford, where grows the great mass of *Lathyrus maritimus*, the seeds of which saved the life of many poor people in a famine in the middle of the sixteenth century, is said to have the greatest breadth of sand anywhere on the English coast. The rivers are shallow streams with slow currents. In the north-east there are several lakes of brackish water, not so well known as the Norfolk Broads, of which Braydon Water covers 1200, and Thorpe Mere 1000, acres. The fresh-water lakes of the county are few and small. There is a considerable area of fen- and marsh-land, both in the north-west and east, so that we get in the county all the conditions that produce a rich low-country flora, and, superadded to the common lowland plants, rarities characteristic of chalk country, the seashore, and fen-land ditches and marshes.

The country is so easy of access from the centres where have lived many of the best botanists of bygone time, such as London, Cambridge, Yarmouth, Norwich, and Saffron Walden, that the principal features of its botany have long been known, and many excellent botanists, from the time of Buddle down to the present day, have resided within its compass. The father of Suffolk botany was Sir John Cullum, F.R.S., who lived near Bury St. Edmunds, and kept a diary between 1772 and 1785, in which he has recorded the occurrence of upwards of 500 plants. To his son, Sir Thomas Cullum, F.R.S., who was also an enthusiastic botanist, Sir J. E. Smith dedicated his "English Flora." In the present work there is not only a full general history of the progress of Suffolk botany, but, under each plant, the name of its first known collector is registered. The first "Flora" of the county was published in 1860. It was carried out mainly by the exertions of the late Mr. E. Skepper, working under the superintendence of Prof. Henslow. After it was published, Mr. Skepper made a great many notes for a new edition, but he died in 1867. For several years the Rev. Churchill Babington, who settled in the county in 1866, paid attention to the subject. In 1875, the Rev. W. M. Hind, a very competent botanist, well known by his "Flora of Harrow," settled in the county, and Dr. Babington sought and obtained his assistance to carry on the work. Dr. Babington died early in the present year.

The bulk of the book is, of course, occupied by the enumeration of the species and an account of the distribution and special localities of the varieties. The county is divided into five districts, and the distribution of the plants is traced through them. Only the Phanerogamia and Vascular Cryptogamia are dealt with, but the mosses of the county have also been well worked.

There is also a detailed tabular comparison of the plants of Suffolk with those of Norfolk, Cambridgeshire, and Essex, and a short chapter on the characteristic plants of the different soils of the county, which will be found very interesting to students of plant-dispersion. The chapters contributed by Dr. Wheeler Hind, the son of the editor, on the geology, physical geography, and meteorology of the county are very full, clear, and add greatly to the interest of the book.

One of the most interesting circumstances in the county flora is the occurrence of several maritime plants far inland. In the Breck country, between Thetford and Mildenhall, grow *Vicia lutea*, *Erythræa littoralis*, *Rumex maritimus*, *Carex arenaria*, *Phleum arenarium*, and *Corynephorus canescens*. These are all seaside plants, and their occurrence fifty miles inland is accounted for by Prof. Newton and the editor by supposing that an arm of the sea has penetrated here southward from the Wash at a comparatively recent period.

It is in Norfolk and Suffolk that the most valuable observations have been made, by Mr. Clement Reid and his fellow-workers, in illustration of the time of origin of our present British flora. The Cromer plant-bed extends into Suffolk, past Pakefield, to Southwold and Dunwich. This is pre-glacial, and yet, out of upwards of forty plants found in it that have been clearly identified, there are only two that are not British now—the spruce fir and *Trapa natans*. At Hoxne, near Diss, lacustrine deposits have been found resting on a bed of boulder clay, but beneath beds which contain bones of the elephant. In these are contained *Salix polaris*, *S. Myrsinites*, *Betula nana*, *Hypnum sarmentosum*, and a *Pinus* which is probably *sylvestris*—all characteristic Arctic-Alpine types, associated with many lowland plants which grow unchanged in Suffolk at the present time. A chapter in the book contains a list of all these plants, but their geological position is not clearly explained.

It will be seen that this is a very interesting and complete county flora, and that it is worthy of being studied carefully by all who are interested in the distribution of our indigenous plants. J. G. B.

THE MANUFACTURE OF IRON AND STEEL.

Iron and Steel Manufacture. By Arthur H. Hiorns. (London: Macmillan and Co., 1889.)

THIS volume is meant as a text-book for beginners, and will very worthily occupy that position. It is full of information, and information of the very kind which the student should possess before entering upon the study of the greater works of Percy or Phillips. On the other hand, those already engaged in the metallurgy of iron and steel will find in these pages much that may be referred to.

The book begins with a brief history of the processes that have been employed down to our own time, the landmarks in which are Dud Dudley's successful attempts to smelt with coal at the beginning of the seventeenth century; Cort's introduction of the puddling process in 1784; Neilson's recommendation to use hot blast in 1828; the revolution produced in the iron trade by the invention of the Bessemer steel process in 1855, as supplemented by R. F. Mushet, of the Siemens furnace and steel process, and finally of Thomas and Gilchrist's basic process.

The chapter which deals with chemical principles and changes, inserted for the benefit of those having a limited knowledge of chemistry, is valuable on account of the simple manner in which it is written; this is particularly the case as regards oxidizing and reducing agents, the examples given of oxidation and reduction showing the reactions very clearly. A chapter is devoted to the definition of metallurgical terms, refractory materials and fuel, another to the ores and alloys of iron, and then a description of the various processes employed in the metallurgy of iron and steel is given, attention being pretty equally divided between the two metals.

The most ancient and most difficult method of extracting iron from the ore is what is known as the direct method, and the author explains clearly the two causes of its failure, whether in the case of the old Catalan or any of the modern processes, and the reason why the blast furnace, although an indirect, has proved so successful a method. These two causes are "the easy oxidation of iron by carbonic acid and water, at the temperature at which ferrous oxide is reduced to the metallic state by carbon, carbonic oxide, or hydrogen, and the facility with which iron at a red heat combines with carbon."

The preparation of the ores for reduction in the blast furnace and their treatment therein are next brought forward, the advantages and disadvantages of the hot blast, the utilization of waste gases, the dimensions and form of blast furnace and subsidiary subjects being treated of.

The metal being now in the state of pig-iron, the means of refining and puddling are described; the various arrangements are set forth by which attempts have been made to effect the work of the puddler by mechanical means, whether by automatic rabbles or rotatory furnaces, and their relative advantages and disadvantages. A chapter is devoted to the treatment of puddled iron under the hammer and in the rolling mill, and to the tinning and galvanizing of iron.

Leaving the subject of malleable iron, the author next considers the question of iron-founding. He describes the cupola furnace in which the pig metal is fused; and the various methods of moulding and casting, and the brands of pig-iron used for different purposes, are treated of.

About a third of the book is devoted to the consideration of steel; it is in this branch of the treatment of iron that the greatest development has occurred of late years, and the book under review treats of all the modern practice. It is pleasant to find, too, in the preparation of an elementary work, that constructive perspective has been employed. Modern processes are not brought into prominence simply because they are modern, and ancient methods are not thrown into the shade if still employed. Amongst the latter we find full attention given to the cementation process, and crucible steel; whilst a chapter is devoted to each of the processes of Bessemer and Siemens. The book finishes with a chapter on steel-casting and on testing.

The volume before us is intended to assist pupils preparing for the ordinary grade examinations of the City and Guilds of London Institute, and its author—the principal of the School of Metallurgy in connection with

the Birmingham and Midland Institute—is to be congratulated on the good work he has done in this connection. The book is illustrated with 72 figures, which agree with the simplicity and clearness of the diction, and questions are found at the end of each chapter, which have been well prepared to test the learner's apprehension of its contents. We are pleased to be able to recommend this little work, as a foundation for the study of the metallurgy of iron and steel.

OUR BOOK SHELF.

On the Creation and Physical Structure of the Earth. By J. T. Harrison, F.G.S., M.Inst.C.E. (London: Longmans, 1889.)

THIS book brings to mind one of the most winning of the vagaries of childhood. A bright child of an inquiring turn will sometimes sit with comical sedateness listening to the talk of its elders. It may afterwards be overheard repeating to one of its playmates, or to some lucky adult who has the knack of winning its confidence, such detached scraps of the conversation as have found a resting-place in its little brain; and, conscious even at its early age of the necessity of some continuity in a narrative, filling up the gaps with inventions or criticisms of its own, charming every way, but mainly on account of their utter want of connection with the subject of the conversation which it is attempting to report. So our author has listened to the teaching of many geologists, and has culled many detached passages from their writings: these he repeats to the world in a book, printing between them comments and lucubrations of his own, about as innocent and as little apposite as the child's prattle—hardly so amusing, however. The following passage is a fair sample of the writer's own share in the book. "The termination of the Secondary Period, which introduced these altered conditions of the surface of the northern hemisphere, was really the commencement of what is called the Glacial epoch in Europe. We have noted signs of glaciation during the deposition of the upper chalk in India and North America, but now the conditions which induced that glaciation are extended in such a manner as to unite these districts, and produce that enormous accumulation of snow and ice at the North Pole, the weight of which in the Miocene epoch depressed the crust in that region and upheaved the mighty mountain ranges to which I have just referred."

The book bristles with cataclysms and catastrophes. The shifting of a thin crust on an internal nucleus which it does not fit, and incessant protrusions of granite, are invoked to account for phenomena which every-day people still persist in thinking are satisfactorily explained by every-day causes. But the author is one born out of due time—two centuries too late. How he and Burnet would have enjoyed a crack together! But there is this to be said, the "Sacred Theory of the Earth" is Burnet's own: the staple of the present work consists of extracts from the works of others. The mottoes are verses from the first chapter of Genesis, but their relevancy to the subject-matter of the chapters which they head is not obvious. A. H. G.

Through Atolls and Islands in the Great South Sea. By F. J. Moss. (London: Sampson Low, 1889.)

MR. MOSS—a member of the House of Representatives, New Zealand—started from Auckland, in September 1886, in the schooner *Buster*, for a voyage among the islands and islets of "the outer lagoon world." He was absent seven months, and during that period he crossed the equator six times, and visited more than forty islands among the least frequented groups. In the present volume he sums up the impressions produced upon him by what he saw and heard in the course of his voyage. Mr. Moss, in dealing with matters which really interest him, shows that he is an accurate observer and a man of sound judgment. His style, although plain and unpretending, is well fitted for the task he has fulfilled. The best parts of the book are those in which he tries to convey some idea of the daily life led by those natives whose customs he had an opportunity of studying. He appreciates warmly some aspects of the various Polynesian types of character, but thinks that the people are likely to degenerate rapidly, unless they can be provided with a better class of native teachers than most of those to whom the duty of guiding them is now intrusted. What is needed, he thinks, is, that the islanders shall have in their work and in their amusements freer scope for the imaginative powers with which they are endowed, and the exercise of which is too often foolishly discouraged. Everything Mr. Moss has to say on this subject deserves the serious consideration of those to whom his warnings and counsels are either directly or indirectly addressed.

LETTERS TO THE EDITOR.

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Who Discovered the Teeth in Ornithorhynchus?

IN NATURE of November 14 (p. 31), Profs. Flower and Latter criticise my note which appeared the week previous (November 7, p. 11), concerning the discovery of teeth in the young *Ornithorhynchus*. They promptly dismiss my claim that Sir Everard Home discovered the teeth of the young *Ornithorhynchus*, by stating that the structures described and figured by Sir Everard are the well-known cornules of the adult animal.

If they will take the trouble to turn to the plate cited by me—namely, Plate lix. of the second volume of Home's "Lectures," 1814—and will read the accompanying explanation, they will see that Home was familiar with the teeth of both the young and the old animal.

For the benefit of those who may not have access to Home's "Lectures," I here reproduce outline tracings of two of his figures. Plate lix. Fig. 2, shows the teeth of the *young Ornithorhynchus*—the "first set," as Home says, "to show that there are two grinding teeth on each side." The next figure is a similar tracing from the succeeding plate in Home's "Lectures" (Plate lx.), which represents, to again use Home's words, "the under jaw of the full-grown *Ornithorhynchus paradoxus*, to show that there is only one grinder on each side." Both of these figures are natural size.

In the face of these *facts*, further comment seems unnecessary.

I admit, of course, that Home did not discover the chemical composition of the teeth of the young animal—this was Poulton's discovery. C. HART MERRIAM.

Washington, D.C., November 30.

[We do not reproduce the outlines sent, as anyone interested in the subject may see the originals, not only in Home's "Comparative Anatomy," but in the Philosophical Transactions, where they first appeared.—ED. NATURE.]

I SHOULD be very sorry to deny the credit of any discovery to Sir Everard Home, or anyone else, if any evidence could be shown of its having been made. Of the figures cited by Dr. Hart Merriam, that of the younger animal seems (as far as can be judged from the roughly executed engraving, with the assistance of the descriptive text) to represent the horny plates, showing the hollows from which the true teeth have recently fallen; that of the old specimen, the same plates after they are fully grown, and their surfaces worn down by attrition. This difference led Home to conjecture that these plates were changed during the growth of the animal—a view which was corrected by Owen ("Comp. Anat. of Vertebrates," vol. iii. p. 272), by the statement

that "each division or tubercle of the [horny] molar is separately developed, and they become confluent in the course of growth." By the way, no one can have been better acquainted with the work of Home than his successor in the Hunterian Chair, Sir Richard Owen; and yet, in his numerous references to this subject (Art. "Monotremata," "Cyclop. Anat. and Physiology"; "Odontography"; "Comp. Anat. of Vertebrates," &c.), no trace is shown of any knowledge of a discovery which could not have failed to have interested him, if it had been made before his time.

If a cursory perusal of Sir Everard Home's first account of the mouth of the Ornithorhynchus (in the Philosophical Transactions for 1800), or any interpretation placed upon his figures, might lead anyone to infer, with Dr. Merriam, that the real teeth of the young animal had been discovered at that time, the best possible authority may be conclusively cited against such an idea, no other than that of Home himself, who, in his later description of the same specimen ("Lectures on Comparative Anatomy," 1814), describes the organs in question as "the first set of *cuticular teeth*"—an expression quite incompatible with their being the teeth described by Mr. Poulton and Mr. Oldfield Thomas. It really seems superfluous to have to remind a zoologist of such high repute as Dr. Hart Merriam that the difference between teeth with the structure and mode of growth which characterize these organs in the Mammalia generally, and the horny epithelial plates of Ornithorhynchus, is not merely one of "chemical composition." W. H. FLOWER.

The Pigment of the Touraco and the Tree Porcupine.

ATTENTION has been lately again directed to the red pigment in the wing feathers of the touraco, which has been stated by several observers to be soluble in pure water. Prof. Church, who was the first to experiment upon this pigment (*The Student*, vol. i., 1868; *Phil. Trans.*, 1869), quotes Mr. Tegetmeier and others, to the effect that this pigment can be washed out of the feathers by water. Later, M. Verreaux (Proc. Zool. Soc., 1871) confirmed these statements from his own experiments while travelling in South Africa; attempting to catch one of these birds whose feathers were sodden with rain, he found that the colour stained his hands "blood-red." A few years ago Prof. Krukenberg ("Vergl. Phys. Studien") took up the study of turacin—as Prof. Church termed the pigment—and added some details of importance to Prof. Church's account; Krukenberg, however, contradicted certain of the statements quoted by Church with reference to the solubility of turacin in pure water, remarking that the pigment in the dead bird is insoluble in water. A writer in the *Standard* of October 17 is able "partially to confirm" the assertion that turacin is soluble in pure water. Seeing that there is some conflict of opinion with regard to this matter, I think it worth while to state that I found it quite easy to extract with tap water (warm) some of the pigment from a spirit-preserved specimen of the bird; only a very small amount could be extracted in this way, and the feathers were not perceptibly decolorized even after remaining in the water for a fortnight. I also experimented upon a feather just shed from one of the specimens now in the Zoological Society's Gardens; this was steeped in water for some time without any effect being visible, but after a period of two days the water became stained a very faint pink.

The touraco, however, is not a unique instance of a terrestrial animal with an external colouring matter soluble in water. I am not aware whether other cases have been recorded, but I find a pigment of a similar kind in a South American tree porcupine (*Sphingurus villosus*).

This porcupine has bright yellow spines which are for the most part concealed by abundant long hair. The spines themselves are parti-coloured, the greater part being tinged with a vivid yellow; the tip is blackish-brown. I was unable to extract this pigment with chloroform, or with absolute alcohol even when heated; like so many other colouring substances which are insoluble in these fluids, the pigment could be extracted by potash or ammonia; I found also that tap water, warm or cold, dissolved out the yellow colour; the action was slower than when the water was first rendered alkaline by the addition of ammonia, but, unlike the touraco, the pigment was nearly, if not quite, as completely dissolved. The skin, from which the spines were taken, was a dried skin of an animal recently living in the Zoological Society's Gardens; it had not been preserved in alcohol or treated in any way which might lead to the supposition that the pigment was chemically altered. There is, therefore, a considerable probability that in the living animal the pigment is also soluble in water. I believe that this yellow pigment is undescribed, but I have not yet completed my study of it; in any case, it is not zoofulvin or picifulvin, or any "lipochrome." FRANK E. BEDDARD.

Exact Thermometry.

IN the account which Prof. Mills has given (NATURE, December 5, p. 100) of M. Guillaume's "Traité pratique de la Thermométrie de précision," the permanent ascent of the zero-point of a mercurial thermometer, after prolonged heating to a high temperature, is stated to be due to compression of the bulb—rendered more plastic by the high temperature—by the external atmospheric pressure.

The constant slow rise of the zero-point of a thermometer at the ordinary temperature is mentioned by Prof. Mills; and the late Dr. Joule's observation of this change in a thermometer during twenty-seven years is specially alluded to. It may, I imagine, be taken for granted that after the lapse of a sufficient length of time—possibly many centuries—a final state of equilibrium would be attained; and it has always appeared to me that the effect of heating the thermometer to a high temperature is simply to increase the rate at which this final state is approached. It is my impression that, owing to the more rapid cooling of the outer parts of the bulb after it has been blown, the inner parts are in a state of tension, as, to a very exaggerated degree, in the Prince Rupert's drops; and that it is the gradual equalization of the tension throughout the glass that causes the contraction; in other words, that the process is one of slow annealing.

This explanation appears to be supported by the facts—(1) that when a thermometer is exposed for a long time to a high temperature, the zero-point rises rapidly at first, then more and more slowly, and finally becomes constant or nearly so; (2) that the higher the temperature the more rapidly is this state of equilibrium attained. I do not know of any experimental evidence that the rate of ascent is influenced by changes of external pressure, and it seemed to be desirable to test the point.

In order to do this I have exposed three thermometers, A, B, and C, constructed by the same maker and of the same kind of glass, to a temperature of about 280° for several days in the same vapour-bath, under the following conditions:—The thermometers were all placed in glass tubes closed at the bottom (C being suspended from above), and the tubes were heated by the vapour of boiling bromonaphthalene. One of the tubes—that containing thermometer C—was exhausted so as to reduce the external pressure on the bulb to zero; the others were open to the air. In thermometer A there was a vacuum over the mercury, but air was admitted into B and C to increase the internal pressure. Consequently, the bulb of A was exposed to a resultant external pressure equal to the difference between the barometric pressure and that of the column of mercury in the stem of the thermometer; the internal and external pressures on the bulb of B were approximately equal; lastly, the internal pressure on the bulb of C was the sum of the pressures of the column of mercury in the stem and of the air above it, while the external pressure was zero.

The following results were obtained:—

	A.	Rise.	B.	Rise.	C.	Rise.
Zero before heating ...	0·15		0·10		−0·10	
		0·35		0·25		0·40
After 2 hours' heating ...	0·50		0·35		0·30	
		0·80		0·75		0·80
After an additional 5½ hours' heating	1·30		1·10		1·10	
Total rise of zero-point...		1·15		1·00		1·20

The thermometers were heated until 5 p.m. each day, and the zero-points read on the following morning.

If the diminution of volume of the thermometer bulb, usually observed, were due to external pressure, the zero-point of A should have risen, that of B should have remained nearly stationary, while that of C should have fallen. Instead of this, however, the zero-points of all three thermometers rose at nearly the same rate; therefore the yielding of the bulbs to pressure, owing to the plasticity of the glass, if it occurred at all, had no sensible effect on the result. SYDNEY YOUNG.

University College, Bristol, December 12.

Locusts in the Red Sea.

A GREAT flight of locusts passed over the s.s. *Golconda* on November 25, 1889, when she was off the Great Hanish Islands in the Red Sea, in lat. 13°·56 N., and long. 42°·30 E.

The particulars of the flight may be worthy of record.

It was first seen crossing the sun's disk at about 11 a.m. as a dense white flocculent mass, travelling towards the north-east at about the rate of twelve miles an hour. It was observed at noon by the officer on watch as passing the sun in the same state of density and with equal speed, and so continued till after 2 p.m.

The flight took place at so high an altitude that it was only visible when the locusts were between the eye of the observer and the sun; but the flight must have continued a long time after 2 p.m., as numerous stragglers fell on board the ship as late as 6 p.m.

The course of flight was across the bow of the ship, which at the time was directed about 17° west of north, and the flight was evidently directed from the African to the Arabian shore of the Red Sea.

The steamship was travelling at the rate of thirteen miles an hour, and, supposing the host of insects to have taken only four hours in passing, it must have been about 2000 square miles in extent.

Some of us on board amused ourselves with the calculation that, if the length and breadth of the swarm were forty-eight miles, its thickness half a mile, its density 144 locusts to a cubic foot, and the weight of each locust $\frac{1}{16}$ of an ounce, then it would have covered an area of 2304 square miles; the number of insects would have been 24,420 billions; the weight of the mass 42,580, millions of tons; and our good ship of 6000 tons burden would have had to make 7,000,000 voyages to carry this great host of locusts, even if packed together 111 times more closely than they were flying.

Mr. J. Wilson, the chief officer of the *Golconda*, permits me to say that he quite agrees with me in the statement of the facts given above. He also states that on the following morning another flight was seen going in the same north-easterly direction from 4.15 a.m. to 5 a.m. It was apparently a stronger brood and more closely packed, and appeared like a heavy black cloud on the horizon.

The locusts were of a red colour, were about $2\frac{1}{2}$ inches long, and $\frac{1}{16}$ of an ounce in weight. G. T. CARRUTHERS.

A Marine Millipede.

IT may interest "D. W. T." (NATURE, December 5, p. 104) to know that *Geophilus maritimus* is found under stones and sea-weeds on the shore at or near Plymouth, and recorded in my "Fauna of Devon," Section "Myriopoda," &c., 1874, published in the Transactions of the Devonshire Association for the Advancement of Literature, Science, and Art, 1874. This species was not known to Mr. Newport when his monograph was written (Linn. Trans., vol. xix., 1845). Dr. Leach has given a very good figure of this species in the *Zoological Miscellany*, vol. iii. pl. 140, Figs. 1 and 2, and says: "Habitat in Britannia inter scopulos ad littora maris vulgatissime." But, so far as my observations go, I should say it is a rare species. See *Zoologist*, 1866, p. 7, for further observations on this animal. EDWARD PARFITT.

Exeter, December 9, 1889.

Proof of the Parallelogram of Forces.

THE objection to Duchayla's proof of the "parallelogram of forces" is, I suppose, admitted by all mathematicians. To base the fundamental principle of the equilibrium of a *particle* on the "transmissibility of force," and thus to introduce the conception of a *rigid body*, is certainly the reverse of logical procedure. The substitute for this proof which finds most favour with modern writers is, of course, that depending on the "parallelogram of accelerations." But this is open to almost as serious objections as the other. For it introduces kinetic ideas which are really nowhere again used in statics. I should therefore propose the following proof, which depends on very elementary geometrical propositions. The general order of argument resembles that of Laplace.

I adopt the "triangular" instead of the "parallelogrammic" form. Thus, if PQ, QR represent in length and direction any directed magnitudes whatever, and, *if these have a single equivalent*, that single equivalent will be represented by PR.

To prove that the equivalent of PQ, QR is PR.

(1) The equivalent of two perpendicular lengths is equal *in length* to their hypothenuse.

For, draw AD perpendicular to hypothenuse BC.

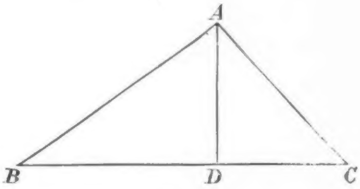

FIG. 1.

Then, let BD, DA $= k$. BA, making angle θ with BA towards BD.

Then, by similar triangles, AD, DC $= k$. AC, making angle θ with AC towards AD.

But these equivalents are at right angles, and proportional to BA and AC. Hence, their equivalent, by similar triangles, is k^2 . BC along BC.

But BD, DA, AD, DC = BC. $\therefore k^2 = 1$; $\therefore k = 1$.

(2) If theorem holds for right-angled triangle containing angle θ, it holds for right-angled triangle containing $\frac{1}{2}\theta$.

For, let ACD $= \theta$, where D is 90°. Produce DC to B, such that CB = CA. Then ABD $= \frac{1}{2}\theta$.

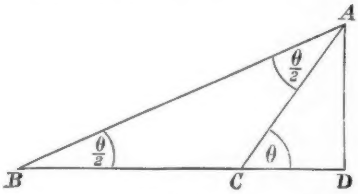

FIG. 2.

Then assume CD, DA = CA. Add BC. $\therefore$ BD, DA = BC, CA.

But BD, DA = BA in magnitude by (1); and BC, CA has its equivalent *along* BA, $\because$ BC = CA. $\therefore$ BD, DA = BA, both in magnitude and direction.

(3) If the theorem holds for θ and ϕ, it holds for $\theta + \phi$.

For make the well-known projection construction. Thus—

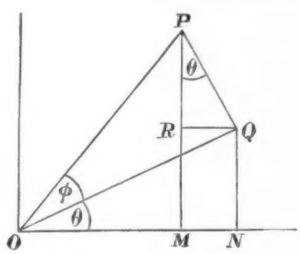

FIG. 3.

$$OP = OQ, QP = ON, NQ, QR, RP = OM, MP.$$

(4) Finally, by (1), theorem holds for isosceles right-angled triangle; $\therefore$ by (2) it holds for right-angled triangle containing angle $90° \div 2^n$; $\therefore$ by (3) it holds for right-angled triangle containing angle $m.\ 90° \div 2^n$: *i.e.* for *any* angle (as may be shown, if considered necessary, by the method for incommensurables in Duchayla's proof).

Hence, if AD be perpendicular on BC in *any* triangle,

$$BA, AC = BD, DA, AD, AC = BC. \quad \text{Q.E.D.}$$

W. E. JOHNSON.

Llandaff House, Cambridge, November 12.

Glories.

Mr. James McConnel asks in Nature (vol. xl. p. 594) for acccounts of the colours and angular dimensions of glories. I saw a good instance of the phenomenon on Lake Superior, June 17, 1888, and, having had my attention called to the value of accurate descriptions in such cases by Mr. Henry Sharpe's "Brocken Spectres," I examined it carefully.

The shadow of my head on the mist was surrounded by a brilliant halo or glory, slaty-white around the head, followed by orange and red; then a circle of blue, green, and red, and the same colours repeated more faintly. The diameter of the innermost and brightest circle of red, as measured on the graduated semicircle of a clinometer, was $4\frac{1}{2}°$. There was also a very distinct, but nearly white, fog-bow outside, of 42° radius, as measured in the same way. A. P. Coleman.

Faraday Hall, Victoria University, Cobourg, Ontario.

Fossil Rhizocarps.

Referring to Sir William Dawson's note on this subject in Nature of November 7 (p. 10), we regret that we have been unable to trace the original source from which the statement in our "Hand-book of Cryptogamic Botany" was derived, relative to the fructification of *Protosalvinia* or *Sporangites*. The sentence will therefore, with apologies to Sir W. Dawson, be removed from future editions of the work.

Alfred W. Bennett.

The Arc-Light.

Would you or any of your readers kindly tell me where I may find an account of any of the latest methods of determining the back E.M.F. of the arc-light? Joseph McGrath.

Mount Sidney, Wellington Place, Dublin.

THE HYDERABAD CHLOROFORM COMMISSION.

The appointment of a Commission at the present time to investigate the action of chloroform as an anæsthetic might to many seem an anomaly. For the use of chloroform as an anæsthetic was introduced over forty years ago: it was in November, 1847, that Prof. Simpson, of Edinburgh, first brought this valuable agent before the medical profession. Since that time, the use of chloroform has enormously extended, especially in our country, and although there are other valuable agents of the same class—such as ether and nitrous-oxide gas—yet there is a universality of opinion that the employment of chloroform has in many cases a special advantage. Considering the extensive use of the agent, and the progress which has been made of late years in the study of the action of drugs in man, it certainly is surprising that the knowledge of the effect of chloroform on the different parts and organs of the body is not complete. This is not altogether from want of attention to the subject; because, previous to the Hyderabad Commission, at least two Commissions were appointed with the view of investigating the action of chloroform and its occasional serious effects. These Commissions were appointed by the Royal Medical and Chirurgical Society of London, and by the British Medical Association, and they were composed of men who, from their knowledge of experiment and acquaintance with practical medicine, were competent to discuss the question. The two Commissions arrived at the same conclusions as the distinguished French man of science, Claude Bernard, had published years before, and these conclusions tallied with the teaching of the great London medical schools.

Chloroform and other anæsthetic agents have a peculiar position: they are powerful drugs used, not for disease itself, but for the purpose of allowing an operation to be performed, preventing the pain which would otherwise be felt, and relaxing the contraction and spasms of the muscles, so that the surgeon can more readily and accurately operate. The administration of the anæsthetic is something, then, outside the diseased condition; so that its use ought theoretically to be perfectly harmless to the sick person. Unfortunately it is not always so, and deaths from chloroform are, although rare, by no means unknown. The administrator of chloroform is therefore a person of great responsibility: he has to watch carefully the effect of the agent on the patient, to notice any unfavourable change that occurs, and to adopt measures to counteract any bad effects which appear. The knowledge of the mode in which chloroform causes danger to the life of the patient is therefore of vast importance; for, if the administrator knows the signs of danger, there is more likelihood of counteracting a fatal result. These fatal results, which are among the saddest that occur in medical practice, ought, if possible, to be avoided.

What, then, is the danger to life of chloroform? Or, to speak more fully, what particular part of the body does chloroform injuriously affect when there is danger? This is just the point that the various Commissions have attempted to settle. In the Scotch schools, more especially that of Edinburgh, it has been taught that the great danger of chloroform was in failure of respiration; meaning by this that the danger-signal of chloroform was the stoppage or irregularity of the breathing. As a corollary to this belief, it was considered that the heart was only affected after the breathing had become interfered with; that, in fact, the respiration stopping, the blood was not oxygenated, so the heart stopped beating. This was the teaching of the great Edinburgh surgeon, Syme. The English (and especially the London) teaching was almost directly opposed to this. It was taught, and is still taught in the London schools, that the great danger from chloroform arose from its effect on the heart, which stopped beating before the respiration ceased. Which, then, of these two doctrines is true, or are both true?

The decision of this question is, as we have stated, one of vast importance; but it must be remembered that, whichever is right, the administrator of anæsthetics always pays attention to both the beating of the heart and the regularity of the respiration. Surgeon-Major Lawrie, one of the prominent members of the Hyderabad Chloroform Commission, says that "it is possible to avert all risk to the heart by devoting the entire attention to the respiration during chloroform administration." Medical opinion in England, both of that of experts (professional anæsthetists) and of the general profession, is distinctly opposed to this view; and the administrator who does not attend to the pulse, as well as to the breathing, is certainly neglecting one of the main paths by which Nature shows us what is going on inside the organism.

From the statement of Surgeon-Major Lawrie just quoted, it will be seen that the Hyderabad Chloroform Commission came to the conclusion that the danger from the administration arose, not from the heart, but from the respiration. This view was strongly combated in our contemporary, the *Lancet*. The importance of the question led the Nizam of Hyderabad to obtain the services of a scientific medical man from England to go out to India and attempt to settle the question. Dr. Lauder Brunton, F.R.S., consented to go; and, well known as he is for his life-long devotion to the experimental investigation of the action of remedies and their practical application, it was considered probable that his aid in the research would lead to interesting and important results. From the somewhat scanty news of the results which have been telegraphed to England, it seems likely that the investigation now progressing at Hyderabad will tend to revolutionize existing views as to the action of chloroform.

Dr. Brunton's views as regards the dangers of chloroform before he left England were clearly expressed in his well-known "Text-book of Pharmacology." In it he says that "the dangers resulting from the employment of

chloroform are death by stoppage of respiration and death by stoppage of the heart;" he lays as much stress on the effect on the heart as on the respiration, and he proceeds to affirm that too strong chloroform vapour may very quickly paralyze the heart. This view is, indeed, similar to the one we have already mentioned as taught in the London schools of medicine. It is also well known that death may occur soon after chloroform has begun to be administered, from the heart being affected. If the operation is begun too soon, fainting from pain may supervene, and a fatal result occur: this has always been strongly insisted upon by Dr. Brunton. Surgeon-Major Lawrie says that in such cases it is not the chloroform that acts on the heart, but simply that there is fatal syncope or fainting.

From the large number of experiments on animals which Dr. Brunton has performed in India, in conjunction with the Hyderabad Commission and a medical delegate of the Indian Government, it appears that the "danger from chloroform is asphyxia or an overdose;" there is none whatever from the heart direct. This statement is a distinct reversal of the view generally held in England. It means that chloroform causes a fatal result by affecting the respiration or by too much being taken into the system and affecting the brain; and that there is no direct paralysis of the heart from the chloroform. A perfectly impartial opinion cannot, however, be formed from the scanty records of the investigation which have been as yet received in England. We must wait for fuller details of the experiments before a final judgment can be passed.

It is well, however, to point out that the prevailing view in England has been founded, not only on experiments on the lower animals, but also on the extended clinical observation of two generations of medical men. Clinical observation is not so accurate or so lucid as that of direct experiment, but it has its value, and one by no means to be despised in a case where it is so extensive, and directed to a subject of such great importance, not only to the medical profession, but to the general public, as the question of the administration of chloroform.

ON THE CAVENDISH EXPERIMENT.

IN the last number of the Proceedings of the Royal Society (vol. xlvi. p. 253), I have given an account of the improvements that I have made in the apparatus of Cavendish for measuring the constant of gravitation. As the principles and some of the details there set out apply very generally to other experiments where extremely minute forces have to be measured, it is possible that an abstract of this paper may be of sufficient interest to find a place in the columns of NATURE.

In the original experiment of Cavendish (Phil. Trans., 1798, p. 469), as is well known, a pair of small masses, *m m* (Fig. 1), carried at the two ends of a very long but light torsion rod, are attracted towards a pair of large masses, M M, thus deflecting the arm until the torsion of the suspending wire gives rise to a moment equal to that due to the attraction. The large masses are then placed on the other side of the small ones, as shown by the dotted circles, and the new position of rest of the torsion arm is determined. Half the angle between the two positions of rest is the deflection produced by the attracting masses. The actual force which must be applied to the balls to produce this deflection, can be directly determined in dynamical units when the period of oscillation and the dimensions and masses of the moving parts are known. In the original experiment of Cavendish, the arm is 6 feet long, the little masses are balls of lead 2 inches in diameter, and large ones are lead balls 1 foot in diameter. Since the attraction of the whole earth on the smaller balls only produces their weight, *i.e.* the force with which they are attracted downwards, it is evident that the balls, M M, which are insignificant in comparison with the size of the earth, can only exert an extremely feeble attraction. So small is this that it can only be detected when the beam is entirely inclosed in a case to protect it from draughts; when, further, the whole apparatus is placed in a room into which no one must enter, because the heat of the body would warm the case unevenly, and so set up air currents which would have far more influence than the whole attraction to be measured; and when, finally, the period of oscillation is made very great, as, for instance, five to fifteen minutes. In order to realize how small must be the force that will only just produce an observable displacement of the balls, *m m*, it is sufficient to remember that the force which brings them back to their position of rest is the same as the corresponding force in the case of a pendulum which swings at the same rate. Now a pendulum that would swing backwards and forwards in five minutes would have to be about 20,000 metres long, so that in this case a deflection of one millimetre would be produced by a force equal to 1/20,000,000 of the weight of the bob. In the case of a pendulum swinging backwards and forwards once in fifteen minutes the corresponding force would be nine times as small, or 1/180,000,000 of the weight.

In spite of the very small value of the constant of gravitation, Cavendish was able, by making the apparatus on this enormous scale, to obtain a couple which

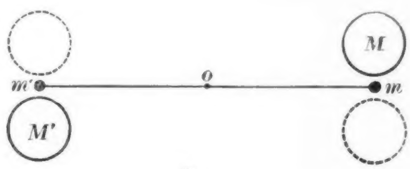

FIG. 1.

would produce a definite deflection against the torsion of his suspending wire.

These measures were repeated by Reich (*Comptes rendus*, 1837, p. 697), and then by Baily (*Phil. Mag.*, 1842, vol. xxi. p. 111), who did not in any important particular improve upon the apparatus of Cavendish, except in the use of a mirror for observing the movements of the beam.

Cornu and Baille (*Comptes rendus*, vol. lxxvi. p. 954, vol. lxxxvi. pp. 571, 699, 1001) have modified the apparatus with satisfactory results. In the first place they have reduced the dimensions of all the parts to about one-quarter of the original amount. Their beam, an aluminium tube, is only ½ metre long, and it carries at its ends masses of ¼ pound each, instead of about 2 pounds, as used by Cavendish. This reduction of the dimensions to about one-quarter of those used previously is considered by them to be one of the advantages of their apparatus, because, as they say, in apparatus geometrically similar, if the period of oscillation is unchanged, the sensibility is independent of the mass of the suspended balls, and *is inversely as the linear dimensions.* I do not quite follow this, because, as I shall show, if all the dimensions are increased or diminished together, the sensibility will be unchanged. If only the length of the beam is altered and the positions of the large attracting masses, so that they remain opposite to, and the same distance from, the ends of the beam, then the sensibility is inversely as the length. This mistake—for mistake it surely is—is repeated in Jamin's "Cours de Physique," tome iv. ed. iv. p. 18, where, moreover, it is emphasized by being printed in italics.

The other improvements introduced by Cornu and

Baille are the use of mercury for the attracting masses which can be drawn from one pair of vessels to the other by the observer without his coming near the apparatus, the use of a metal case connected with the earth to prevent electrical disturbances, and the electrical registration of the movements of the index on the scale, which they placed 560 centimetres from the mirror.

The great difficulty that has been met with has been the perpetual shifting of the position of rest, due partly to the imperfect elasticity or fatigue of the torsion wires, but chiefly, as Cavendish proved experimentally, to the enormous effects of air-currents set up by temperature differences in the box, which, with large apparatus, it is impossible to prevent. In every case the power of observing was in excess of the constancy of the effect actually produced. The observations of Cornu are the only ones which are comparable in accuracy with other physical measurements, and these, as far as the few figures given enable one to judge, show a very remarkable agreement between values obtained for the same quantity from time to time.

Soon after I had made quartz fibres, and found their value for producing a very small and constant torsion, I thought that it might be possible to apply them to the Cavendish apparatus with advantage. Prof. Tyndall, in a letter to a neighbour, expressed the conviction that it would be possible to make a much smaller apparatus in which the torsion should be produced by a quartz fibre. The result of an examination of the theory of the instrument shows that very small apparatus ought practically to work, but that in many particulars there is an advantage in departing from the arrangement which has always been employed, conclusions which experiment has fully confirmed.

As I have already stated, the sensibility of the apparatus is, if the period of oscillation is always the same, independent of its linear dimensions. Thus, if there are two instruments in which all the dimensions of one are n times the corresponding dimensions of the other, the moment of inertia of the beam and its appendages will be as $n^5 : 1$, and, therefore, the torsion also must be as $n^5 : 1$. The attracting masses, both fixed and movable, will be as $n^3 : 1$, and their distance apart as $n : 1$. Therefore, the attraction will be as n^6/n^2 or $n^4 : 1$, and this is acting on an arm n times as long in the large instrument as in the small; therefore the moment will be as $n^5 : 1$; that is, in the same proportion as the torsion, and so the angle of deflection is unchanged.

If, however, the length of the beam only is changed, and the attracting masses are moved until they are opposite to, and a fixed distance from, the ends of the beam, then the moment of inertia will be altered in the ratio $n^2 : 1$, while the corresponding moment will only change in the ratio of $n : 1$; and thus there is an advantage in reducing the length of the beam until one of two things happens: either it is difficult to find a sufficiently fine torsion thread that will safely carry the beam and produce the required period—and this, I believe, has up to the present time prevented the use of a beam less than $\frac{1}{2}$ metre in length—or else, when the length becomes nearly equal to the diameter of the attracting balls, they then act with such an increasing effect on the opposite suspended balls, so as to tend to deflect the beam in the opposite direction, that the balance of effect begins to fall short of that which would be due to the reduced length if the opposite ball did not interfere. Let this shortening process be continued until the line joining the centres of the masses M M makes an angle of 45° with the line $m\,m$; then, without further moving the masses M M, a still greater degree of sensibility can be obtained, provided the period remains unaltered, by reducing the length of the beam $m\,m$ to half its amount, so that the distance between the centres of M M is $2\sqrt{2}$ times the new length $m\,m$, at which point a maximum is reached.

It might be urged against this argument that a difficulty would arise in finding a torsion fibre that would give to a very short beam, loaded with balls that it will safely carry, a period as great as five or ten minutes, and until quartz fibres existed there would have been a difficulty in using a beam much less than a foot long, but it is now possible to hang one only half an inch long and weighing from twenty to thirty grains by a fibre not more than a foot in length, so as to have a period of five minutes. If the moment of inertia of the heaviest beam of a certain length that a fibre will safely carry is so small that the period is too rapid, then the defect can be remedied by reducing the weight, for then a finer fibre can be used, and since the torsion varies approximately as the square of the strength (not exactly, because fine fibres carry heavier weights in proportion), the torsion will be reduced in a higher ratio, and so by making the suspended parts light enough, any slowness that may be required may be provided.

Practically, it is not convenient to use fibres much less than one ten-thousandth of an inch in diameter, and these have a torsion 10,000 times less than that of ordinary spun glass. A fibre one five-thousandth of an inch in diameter will carry a little over thirty grains.

Since with such small apparatus as I am now using it is easy to provide attracting masses which are very large in proportion to the length of the beam, while with large apparatus comparatively small masses must be made use of owing to the impossibility of dealing with balls of lead of great size, it is clear that much greater deflections can be produced with small than with large apparatus. For instance, to get the same effect in the same time from an instrument with a 6-foot beam that I get from one in which the beam is five-eighths of an inch long, and the attracting balls are 2 inches in diameter, it would be necessary to provide and deal with a pair of balls each 25 feet in diameter, and weighing 730 tons instead of about $1\frac{3}{4}$ pound apiece. There is the further advantage in small apparatus that if for any reason the greatest possible effect is desired, attracting balls of gold would not be entirely unattainable, while such small masses as two piles of sovereigns could be used where qualitative effects only were to be shown. Owing to its strongly magnetic qualities, platinum is unsuited for experiments of this kind.

By far the greatest advantage that is met with in small apparatus is the perfect uniformity of temperature which is easily obtained, whereas, with apparatus of large size, this alone makes really accurate work next to impossible. The construction to which this inquiry has led me, and which will be described later, is especially suitable for maintaining a uniform temperature in that part of the instrument in which the beam and mirror are suspended.

With such small beams as I am now using it is much more convenient to replace the long thin box generally employed to protect the beam from disturbance by a vertical tube of circular section, in which the beam with its mirror can revolve freely. This has the further advantage that, if the beam is hung centrally, the attraction of the tube produces no effect, and the troublesome and approximate calculations which have been necessary to find the effect of the box are no longer required. The attracting weights, which must be outside the tube, must be made to take alternately positions on the two sides of the beam, so as to deflect it first in one direction and then in the other. For this purpose they are most conveniently fastened to the inside of a larger metal tube, which can be made to revolve on an axis coincident with the axis of the smaller tube. There are obviously two planes, one containing and one at right angles to the beam, in which the centres of the attracting balls will lie when they produce no deflection. At some intermediate position the deflection will be a maximum. Now, it is a matter of some importance to choose this maximum

position for the attracting masses, because, in showing the experiment to an audience, the largest effect should be obtained that the instrument is capable of producing; while in exact measures of the constant of gravitation this position has the further advantage that the only measurement which there is any difficulty in making, viz. the angle between the line joining the large masses and the line joining the small, which may be called the azimuth of the instrument, becomes of little consequence under these circumstances. In the ordinary arrangement the slightest uncertainty in this angle will produce a relatively large uncertainty in the result. I have already stated that if an angle of 45° is chosen, the distance between the centres of the large balls should be $2\sqrt{2}$ times the length of the beam, and the converse of course is true. As it would not be possible at this distance to employ attracting balls with a diameter much more than one and a half times the length of the beam, and as balls much larger than this are just as easily made and used, I have found by calculation what are the best positions when the centres of the attracting balls are any distance apart.

If the effect on the nearer ball only is considered, then it is easy to find the best position for any distance of the attracting mass from the axis of motion. Let P (Fig. 2) be the centre of the attracting ball, N that of the nearer

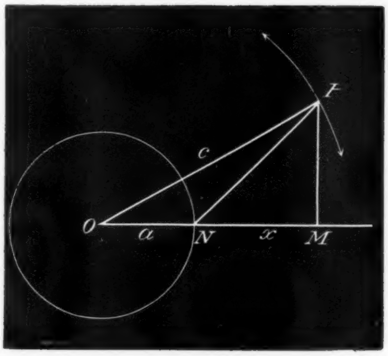

FIG. 2.

attracted ball, O the axis of motion, c and a the distances of P and N from O, and x the distance from N of the foot of the perpendicular from P on ON produced. Then the moment of N about O will be greatest when

$$x^2 + \frac{3a^2 + c^2}{a} x = 2(c^2 - a^2),$$

or what comes to the same thing when

$$\cos^2 \theta + \frac{c^2 + a^2}{ca} \cos \theta = 3.$$

Now, as the size of the attracting masses M M is increased, or, as is then necessarily the case, as the distance of their centres from the axis increases, their relative action on the small masses $m\,m$ at the opposite ends of the beam increases, and so but a small fraction of the advantage is obtained, which the large balls would give if they acted only upon the small balls on their own side. For instance, if the distance between the centres of M M is five times the length of the beam, the moment due to the attraction on the opposite small balls is nearly half as great as that on the near balls, so that the actual sensibility is only a little more than half that which would be obtained if the cross action could be prevented.

I have practically overcome this difficulty by arranging the two sides of the apparatus at different levels. Each large mass is at or near the same level as the neighbouring small one, but one pair is removed from the level of the other by about the diameter of the large masses which in the apparatus figured below is nearly five times as great as the distance *in plan* between the two small masses.

In order to realize more fully the effect of a variety of arrangements, I have, for my own satisfaction, calculated the values of the deflecting forces in an instrument in which the distance between the centres of the attracting balls is five times the length of the beam, for every azimuth and for differences of levels of 0, 1, 2, 3, 4, and 5 times the length of the beam.

The result of the calculation is illustrated by a series of curves in the original paper. The main result, however, is this.

In the particular case which I have chosen for the instrument, *i.e.* where the distance between the centres of M M and the axis, and the difference of level between the two sides are both five times the length of the beam, as seen in plan, and where the diameter of the large masses is 6·4 times the length of the beam, the angle of deflection becomes 18·7 times as great as the corresponding angle in the apparatus of Cavendish, provided that the large masses are made of material of the same density in the two cases and the periods of oscillation are the same.

Having now found that with apparatus no bigger than an ordinary galvanometer it should be possible to make an instrument far more sensitive than the large apparatus in use heretofore, it is necessary to show that such a piece of apparatus will practically work, and that it is not liable to be disturbed by the causes which in large apparatus have been found to give so much trouble.

I have made two instruments, of which I shall only describe the second, as that is better than the first, both in design and in its behaviour.

The construction of this is made clear by Fig. 3. To a brass base provided with levelling screws is fixed the vertical brass tube t, which forms the chamber in which the small masses $a\,b$ are suspended by a quartz fibre from a pin at the upper end. These little masses are cylinders[1] of pure lead 11·3 millimetres long and 3 millimetres in diameter, and the vertical distance between their centres is 50·8 millimetres. They are held by light brass arms to a very light taper tube of glass, so that their axes are 6·5 millimetres from the axis of motion. The mirror m, which is 12·7 millimetres in diameter, plane, and of unusual accuracy, is fastened to the upper end of the glass tube by the smallest quantity of shellac varnish. Both the mirror and the plate-glass window which covers an opening in the tube were examined, and afterwards fixed with the refracting edge of each horizontal, so that the slight but very evident want of parallelism between their faces should not interfere with the definition of the divisions of the scale. The large masses M M are two cylinders[1] of lead 50·8 millimetres in diameter, and of the same length. They are fastened by screws to the inside of a brass tube, the outline of which is dotted in the figure, which rests on the turned shoulder of the base, so that it may be twisted without shake through any angle. Stops (not shown in the figure) are screwed to the base, so that the actual angle turned through shall be that which produces the maximum deflection. A brass lid made in two halves covers in the outer tube, and serves to maintain a very perfect uniformity of temperature in the inner tube. Neither the masses M M, nor the lid, touch the inner tube. The period of oscillation is 160 seconds.

With this apparatus placed in an ordinary room with

[1] Cylinders were employed instead of spheres, because they are more easily made and held, and because spheres have no advantage except when absolute calculations have to be made. Also the vertical distance $a\,b$ was for convenience made only about four times the length $a\,b$ in plan.

Baille are the use of mercury for the attracting masses which can be drawn from one pair of vessels to the other by the observer without his coming near the apparatus, the use of a metal case connected with the earth to prevent electrical disturbances, and the electrical registration of the movements of the index on the scale, which they placed 560 centimetres from the mirror.

The great difficulty that has been met with has been the perpetual shifting of the position of rest, due partly to the imperfect elasticity or fatigue of the torsion wires, but chiefly, as Cavendish proved experimentally, to the enormous effects of air-currents set up by temperature differences in the box, which, with large apparatus, it is impossible to prevent. In every case the power of observing was in excess of the constancy of the effect actually produced. The observations of Cornu are the only ones which are comparable in accuracy with other physical measurements, and these, as far as the few figures given enable one to judge, show a very remarkable agreement between values obtained for the same quantity from time to time.

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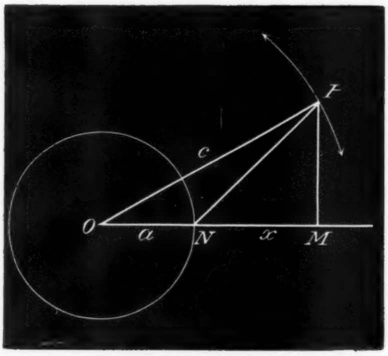

FIG. 2.

attracted ball, O the axis of motion, c and a the distances of P and N from O, and x the distance from N of the foot of the perpendicular from P on ON produced. Then the moment of N about O will be greatest when

$$x^2 + \frac{3a^2 + c^2}{a}x = 2(c^2 - a^2),$$

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$$\cos^2\theta + \frac{c^2 + a^2}{ca}\cos\theta = 3.$$

Now, as the size of the attracting masses M M is increased, or, as is then necessarily the case, as the distance of their centres from the axis increases, their relative action on the small masses *m m* at the opposite ends of the beam increases, and so but a small fraction of the advantage is obtained, which the large balls would give if they acted only upon the small balls on their own side. For instance, if the distance between the centres of M M is five times the length of the beam, the moment due to the attraction on the opposite small balls is nearly half as great as that on the near balls, so that the actual sensibility is only a little more than half that which would be obtained if the cross action could be prevented.

I have practically overcome this difficulty by arranging the two sides of the apparatus at different levels. Each large mass is at or near the same level as the neighbouring small one, but one pair is removed from the level of the other by about the diameter of the large masses which in the apparatus figured below is nearly five times as great as the distance *in plan* between the two small masses.

In order to realize more fully the effect of a variety of arrangements, I have, for my own satisfaction, calculated the values of the deflecting forces in an instrument in which the distance between the centres of the attracting balls is five times the length of the beam, for every azimuth and for differences of levels of 0, 1, 2, 3, 4, and 5 times the length of the beam.

The result of the calculation is illustrated by a series of curves in the original paper. The main result, however, is this.

In the particular case which I have chosen for the instrument, *i.e.* where the distance between the centres of M M and the axis, and the difference of level between the two sides are both five times the length of the beam, as seen in plan, and where the diameter of the large masses is 6·4 times the length of the beam, the angle of deflection becomes 18·7 times as great as the corresponding angle in the apparatus of Cavendish, provided that the large masses are made of material of the same density in the two cases and the periods of oscillation are the same.

Having now found that with apparatus no bigger than an ordinary galvanometer it should be possible to make an instrument far more sensitive than the large apparatus in use heretofore, it is necessary to show that such a piece of apparatus will practically work, and that it is not liable to be disturbed by the causes which in large apparatus have been found to give so much trouble.

I have made two instruments, of which I shall only describe the second, as that is better than the first, both in design and in its behaviour.

The construction of this is made clear by Fig. 3. To a brass base provided with levelling screws is fixed the vertical brass tube *t*, which forms the chamber in which the small masses *a b* are suspended by a quartz fibre from a pin at the upper end. These little masses are cylinders[1] of pure lead 11·3 millimetres long and 3 millimetres in diameter, and the vertical distance between their centres is 50·8 millimetres. They are held by light brass arms to a very light taper tube of glass, so that their axes are 6·5 millimetres from the axis of motion. The mirror *m*, which is 12·7 millimetres in diameter, plane, and of unusual accuracy, is fastened to the upper end of the glass tube by the smallest quantity of shellac varnish. Both the mirror and the plate-glass window which covers an opening in the tube were examined, and afterwards fixed with the refracting edge of each horizontal, so that the slight but very evident want of parallelism between their faces should not interfere with the definition of the divisions of the scale. The large masses M M are two cylinders[1] of lead 50·8 millimetres in diameter, and of the same length. They are fastened by screws to the inside of a brass tube, the outline of which is dotted in the figure, which rests on the turned shoulder of the base, so that it may be twisted without shake through any angle. Stops (not shown in the figure) are screwed to the base, so that the actual angle turned through shall be that which produces the maximum deflection. A brass lid made in two halves covers in the outer tube, and serves to maintain a very perfect uniformity of temperature in the inner tube. Neither the masses M M, nor the lid, touch the inner tube. The period of oscillation is 160 seconds.

With this apparatus placed in an ordinary room with

[1] Cylinders were employed instead of spheres, because they are more easily made and held, and because spheres have no advantage except when absolute calculations have to be made. Also the vertical distance *a b* was for convenience made only about four times the length *a b* in plan.

draughts of air of different temperatures and with a lamp and scale such as are used with a galvanometer, the effect of the attraction can easily be shown to a few, or, with a lime-light, to an audience. To obtain this result with apparatus of the ordinary construction and usual size is next to impossible, on account chiefly of the great disturbing effect of air currents set up by difference of temperature in the case. The extreme portability of the new instrument is a further advantage, as is evident when the enormous weight and size of the attracting masses in the ordinary apparatus are considered.

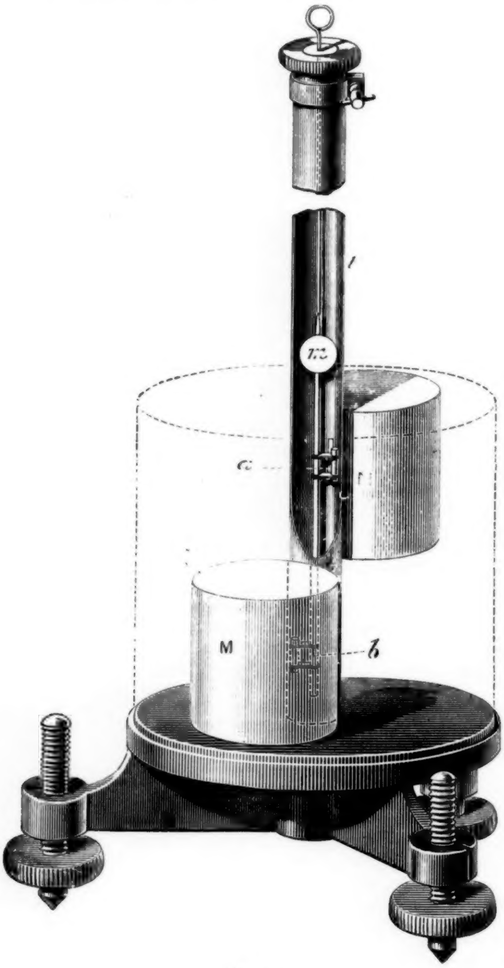

Fig. 3.

However, this result is only one of the objects of the present inquiry. The other object which I had in view was to find whether the small apparatus, besides being more sensitive than that hitherto employed, would also be more free from disturbances and so give more consistent results. With this object I have placed the apparatus in a long narrow vault under the private road between the South Kensington Museum and the Science Schools. This is not a good place for experiments of this kind, for when a cab passes overhead the trembling is so great that loose things visibly move; however, it is the only place at my disposal that is in any degree suitable. A large drain-pipe filled with gravel and cement and covered by a slab of stone forms a fairly good table. The scale is made by etching millimetre divisions on a strip of clear plate glass 80 centimetres long. This is secured at the other end of the vault at a distance of 1053·8 centimetres from the mirror of the instrument. A telescope 132 centimetres long with an object-glass 5·08 centimetres in diameter, rests on V's clamped to the wall, with its object-glass 360 centimetres from the mirror. Thus any disturbance that the observer might produce if nearer is avoided, and at the same time the field of view comprises 100 divisions. While the observer is sitting at the telescope he can, by pulling a string, move an albo-carbon light, mounted on a carriage, so as to illuminate any part of the scale that may happen to be in the field of the telescope. The white and steady flame forms a brilliant background on which the divisions appear in black. The accuracy of the mirror is such that the millimetre divisions are clearly defined, and the position of the cross-wire (a quartz fibre) can be read accurately to one-tenth of a division. This corresponds to a movement of the mirror of almost exactly one second of arc.

The mode of observation is as follows: When all is quiet with the large masses in one extreme position, the position of rest is observed and a mark placed on the scale. The masses are moved to one side for a time and then replaced, which sets up an oscillation. The reading of every elongation and the time of every transit of the mark are observed until the amplitude is reduced to 3 or 4 centimetres. The masses are then moved to the other extreme position and the elongations and transits observed again, and this is repeated as often as necessary.

On the evening of Saturday, May 18, six sets of readings were taken, but during the observations there was an almost continuous tramp of art students above, producing a perceptible tremor, besides which two vehicles passed, and coals were twice shovelled in the coal cellar, which is separated from the vault in which the observations were made by only a 4½-inch brick wall. The result of all this was a nearly perpetual tremor, which produced a rapid oscillation of the scale on the cross-wire, extending over a little more than 1 millimetre. This increased the difficulty of taking the readings, but to what extent it introduced error I shall not be able to tell until I can make observations in a proper place.

In spite of these disturbances, the agreement between the deflections deduced from the several sets of observations, and between the periods, is far greater than I had hoped to obtain, even under the most favourable conditions. In order to show how well the instrument behaved, I have copied from my note-book the whole series of figures of one set, which sufficiently explain themselves.

Elongation.	Amplitude.	Decrement.	True Posit'on of Rest.	Time of Transit of 36·09.	Correction for Transit of True Position of Rest.	True Half Period.
15·05				h. m. s.		
53·20	38·15	0·805	36·18	9 8 25·0	+ 0·08	80·2
22·48	30·72	0·808	36·20	9 45·5	− 0·18	80·2
47·28	24·80	0·807	36·21	11 5·3	+ 0·24	80·0
27·28	20·00	0·807	36·20	12 25·8	− 0·28	79·9
43·40	16·12	0·805	36·22	13 45·0	+ 0·41	80·1
30·42	12·98	0·806	36·21	15 6·0	− 0·47	80·1
40·88	10·46	0·802	36·22	16 25·0	+ 0·63	79·5
32·50	8·38	0·808	36·24	17 46·0	− 0·91	80·5
39·27	6·77	0·808	36·24	19 4·5	+ 1·13	79·8
33·80	5·47	0·814	36·26	20 27·0	− 1·58	80·5
38·25	4·45		36·26	21 44·0	+ 1·94	
		0·8066				80·08

It will be noticed that the true position of rest is slightly rising in value, and this rise was found to continue at the rate of 0·36 centimetre an hour during the whole course of the experiment, and to be the same when the large masses were in the positive or negative position. The motion was perfectly uniform, and in no way interfered with the accuracy of the experiments. It was due, I believe, to the shellac fastening of the fibre, for I find that immediately after a fibre has been attached, this movement is very noticeable, but after a few days it almost entirely ceases; it is, moreover, chiefly evident when the fibre is loaded very heavily. At the time that the experiment was made the instrument had only been set up a few hours.

The mean decrement of three positive sets was 0·8011, and of three negative sets, 0·8035. The observed mean period of three positive sets was 79·98, and of three negative sets, 80·03 seconds, from both of which 0·20 must be deducted as the time correction for damping.

The deflections, in centimetres, obtained from the six sets of observations taken in groups of three, so as to take into account the effect of the slow change of the position of rest, were as follows:—

From sets 1, 2, and 3	...	17·66 ± 0·01
,, 2, 3, and 4	...	17·65 ± 0·02
,, 3, 4, and 5	...	17·65 ± 0·02
,, 4, 5, and 6	...	17·65 ± 0·02

An examination of these figures shows that the deflection is known with an accuracy of about one part in two thousand, while the period is known to the 4000th part of the whole. As a matter of fact, the discrepancies are not more than may be due to an uncertainty in some of the observations of ½ millimetre or less, a quantity which, under the circumstances, is hardly to be avoided.

The result of these experiments is complete and satisfactory. As a lecture experiment, the attraction between small masses can be easily and certainly shown, even though the resolved force causing motion is, as in the present instance, no more than the 1/200,000 of a dyne (less than 1/10,000,000 of the weight of a grain), and this is possible with the comparatively short half period of 80 seconds. Had it been necessary to make use of such half periods as three to fifteen minutes, which have been employed hitherto, then, even though a considerable deflection were produced, this could hardly be considered a lecture experiment. So perfectly does the instrument behave, that there can be no difficulty in making a fairly accurate measure of the attraction between a pair of No. 5, or, I believe, even of dust shot.

The very remarkable agreement between successive deflections and periods shows that an absolute measure made with apparatus designed for the purpose, but on the lines laid down above, is likely to lead to results of far greater accuracy than any that have been obtained. For instance, in the original experiment of Cavendish there seems to have been an irregularity in the position of rest of one-tenth of the deflection obtained, while the period showed discrepancies of five to fifteen seconds in seven minutes. The experiments of Baily, made in the most elaborate manner, were more consistent, but Cornu was the first to obtain from the Cavendish apparatus results having a precision in any way comparable to that of other physical measurements. The three papers, published by him in the *Comptes rendus* of 1878, referred to above, contain a very complete solution of some of the problems to which the investigation has given rise. The agreement between the successive values, decrement, and period is much the same as I have obtained, nevertheless the means of the summer and of the winter observations differ by about 1 per cent.

I have not referred to the various methods of determining the constant of gravitation in which a balance, whether with the usual horizontal beam, or with a vertical beam on the metronome principle, is employed. They are essentially the same as the Cavendish method, except that there is introduced the friction of the knife-edges and the unknown disturbances due to particles of dust at these points, and to buoyancy, without, in my opinion, any compensating advantage. However, it would appear that if the experiment is to be made with a balance, the considerations which I have advanced in this paper would point to the advantage of making the apparatus small, so that attracting masses of greater proportionate size may be employed, and the disturbance due to convection reduced.

It is my intention, if I can obtain a proper place in which to make the observations, to prepare an apparatus specially suitable for absolute determinations. The scale will have to be increased, so that the dimensions may be determined to a ten-thousandth part at least. Both pairs of masses should, I think, be suspended by fibres or by wires, so that the distance of their centres from the axis may be accurately measured, and so that, in the case of the little masses, the moment of inertia of the beam, mirror, &c., may be found by alternately measuring the period with and without the masses attached. The unbalanced attractions between the beam, &c., and the large masses, and between the little masses and anything unsymmetrical about the support of the large masses, will probably be more accurately determined experimentally by observing the deflections when the large and the small masses are in turn removed, than by calculation.

If anything is to be gained by swinging the small masses in a good Sprengel vacuum, the difficulty will not be so great with apparatus made on the scale I have in view, *i.e.* with a beam about 5 centimetres long, as it would with large apparatus. With a view to reduce the considerable decrement, I did try to maintain such a vacuum in the first instrument, in which a beam 1·2 centimetre long was suspended by a fibre so fine as to give a complete period of five minutes, but though the pump would click violently for a day perhaps, leakage always took place before long, and so no satisfactory results were obtained.

With an apparatus such as I have described, but arranged to have a complete period of six minutes, it will be possible to read the scale with an accuracy of 1/10,000 of the deflection, and to determine the time of vibration with an accuracy about twice as great.

I hope early next year, in spite of the difficulty of finding a suitable place to observe in, to prepare apparatus for absolute determinations, and I shall be glad to receive any suggestions which those interested may be good enough to offer.

C. V. BOYS.

WILLIAM RAMSAY McNAB.

WILLIAM RAMSAY McNAB, M.D., whose sudden death from heart-disease we have already recorded, was born in Edinburgh in November 1844. He was educated at the Edinburgh Academy, and afterwards in the University of that city, obtaining the degree of Doctor of Medicine when twenty-two years of age.

His grandfather and father, in succession, held office as Curators of the Edinburgh Botanic Garden; and the late Dr. McNab early manifested an inherited capacity for botanical work; for, while still an undergraduate, he was appointed assistant to Prof. Balfour, who then held the Edinburgh botanical chair. He also entered the University of Berlin as a student—in botany under Profs. Braun and Koch, and in pathological anatomy and histology under Prof. Virchow. Three years of his later life were spent in medical practice; but his love of botany was his dominant feeling, and in 1870 he embarked upon a purely biological career, having been then appointed to the Professorship of Natural History

in the Royal Agricultural College, Cirencester. Two years later he succeeded to the Chair of Botany in the Royal College of Science, Dublin, and this post he held until his death. During his student life he paid considerable attention to the practical study of geology; and for many years he collected Coleoptera, of which he possessed a very fine collection, now in the Dublin Museum of Science and Art.

During the nineteen years exclusively devoted to natural science, Prof. McNab published a considerable number of technical papers; most of these were short, but some forty or fifty of them are fit to rank as original communications. The work by which he is best known was that upon the movements of water in plants. Following a suggestion of Prof. A. H. Church, that lithium might prove useful in his researches, he instituted experiments which proved the value of this method, and paved the way for later investigators. McNab's chief claim to distinction lay, however, not in the direction of pure research, but in the fact of his having been the first to introduce to British students the methods of Sachs, now universally adopted. He inaugurated the modern methods of teaching botany at Cirencester, in the year 1871, and at Dublin two years later; and he fully admitted his indebtedness to the first edition of Sachs's celebrated "Lehrbuch der Botanik." Dr. McNab was, at the time of his death, an examiner in botany to the Victoria University, Manchester. The appointment of Scientific Superintendent of the Royal Botanic Gardens, Glasnevin, Dublin, was created for him in 1880, and in connection with this office he issued, five years later, an enlarged and considerably revised Guide-book. He was joint author, with Prof. Alex. Macalister, of a "Guide-book to the County of Dublin," prepared on the occasion of the visit of the British Association to that city. In 1878 he published, in Longmans' "London Science Series," two botanical class-books, entitled "Outlines of Morphology and Physiology," and "Outlines of Classification"; and he leaves behind him the first few chapters, and a large amount of manuscript in a nearly completed condition, of a contemplated "Text-book of Botany," which he was to have written for Messrs. C. Griffin and Co. In 1888 he was appointed Swiney Lecturer to the British Museum of Natural History, and in that capacity he has lectured for two sessions. His discourses, which were upon "The Fossil Plants of the Palæozoic Epoch" and "Ferns and Gymnosperms of the Palæozoic and Mesozoic Epochs, and dawn of the Angiospermous Flora" respectively, were attended with much success. He has left behind him carefully written manuscript lectures, which it is sincerely hoped may be published as a memorial volume. At the time of his decease he was actively engaged upon his intended third course, in which he would have dealt with the Cainozoic flora. He was an excellent teacher, possessed of a natural aptitude for the work; and his laboratory instruction was characterized by thoroughness and precision. As a lecturer he was fluent and entertaining; and, in his several capacities, he endeared himself to those with whom he came in contact. Friends, colleagues, and students, alike mourn his loss.

NOTES.

THE death of Prof. Lorenzo Respighi, Director of the Osservatorio Campidoglio, Rome, which we deeply regret to announce, is a great loss to science. He died on December 10.

IN a recent number we gave some account of a meeting held in Manchester on November 25 for the purpose of preparing the way for the erection of a memorial of James Prescott Joule in that city. It was resolved that the memorial should be in the form of a white marble statue, and a committee was appointed to carry out this resolution. At the first meeting of the committee, on November 29, an executive committee was appointed, and the following motion was adopted:—"That the movement be directed to secure, not only a marble statue of the late Dr. James Prescott Joule as a companion to that of the late Dr. Dalton by Sir Francis Chantrey, but also a replica in bronze to occupy some public place in the city, and that the executive committee be instructed to take all needful steps for that purpose." Many subscriptions have been already promised.

AN attempt is being made to secure the erection of an international monument to James Watt at Greenock, his birthplace. It is proposed that the memorial shall be "a large and thoroughly equipped technical school."

A NEW fortnightly scientific periodical is about to be published in Paris. It will be entitled *Revue Générale des Sciences Pures et Appliquées*, and will deal with the mathematical, physical, and natural sciences, and with their applications in geodesy, navigation, engineering, manufactures, agriculture, hygiene, medicine, and surgery. According to the preliminary statement, the new periodical will take as its model the method of exposition adopted in NATURE. The editor is M. Louis Olivier, and the list of contributors includes many of the most eminent French men of science. The first number will appear on January 15, 1890.

THE second Report of the Committee appointed by the British Association to inquire into, and report upon, the present methods of teaching chemistry, which was presented at the Newcastle meeting, and to which we called attention in these columns a short time ago, has now been put on sale by the Council. It may be obtained from the office of the Association, 22 Albemarle Street, W.

ON Tuesday evening, after the distribution of the prizes and certificates to the students of the City and Guilds of London Institute, at Goldsmiths' Hall, Sir Henry Roscoe congratulated the students of the various schools upon the reports he had heard. He observed that the City Guilds were now engaged separately and collectively in nobly carrying out the work for which they were, to a certain extent, originally founded. The Technical Instruction Bill which was passed in the last session of Parliament had materially changed the whole aspect of affairs, and sooner or later a complete scheme for technical education would have to be framed. The beginning of such a scheme had been made by the efforts of the City of London Institution, which, with its many branches, was a nucleus of such a system, the importance of which would only be recognized when the history of that important movement came to be written. It was a satisfactory thing to hear that employers of skilled labour were beginning to find out that the men who had been trained at such Colleges as these were of greater value than those who had not received such training. It was not only necessary to educate the craftsman; the employer needed it equally, if not more. He thought that the Council of the Institute had fully recognized that fact at their Central Institution, but a demand for high-class education had yet to be created.

THE *British Medical Journal* says that owing to the somewhat late period in the year at which the invitation to hold the annual meeting of the British Medical Association in Birmingham was received and accepted, the arrangements are not yet so complete as in former years; but a large general committee and all the necessary sub-committees have been formed, and the use of the requisite public buildings has been obtained.

ON March 1, 1890, a new marine laboratory will be opened at Saint-Wast-la-Hougue.

WE are glad to know that there will soon be well-equipped physical and chemical laboratories at Bedford College, London. Mr. Tate, who has already given £1000 towards the new College buildings, which are on the eve of completion, has

offered a second £1000 towards the fitting up and equipment of the laboratories, contingent on the friends of the College contributing an equal amount. We purpose shortly giving an account and plans of these laboratories.

MORE than a quarter of a century has passed since it was decided that the *Entomologist's Monthly Magazine* should be started. The editors have now resolved to issue a new series, each volume of which will begin in January and end in December. There will be no radical change in the constitution of the magazine, but the number of pages and illustrations will often be increased.

THE result of the poll for a free library at Whitechapel, declared last Saturday night, is interesting and significant. On a register of 6000, there were 3553 affirmative votes and only 935 dissentients. This is the more noteworthy, because about eleven years ago a like proposal was rejected by a majority of about two to one.

THE following science lectures will be given at the Royal Victoria Hall during January: January 7, "A Visit to the Chief Cities of Italy," by Rev. W. W. Edwards; January 14, "The Bottom of the Sea," by Dr. P. H. Carpenter; January 21, "To Vancouver's Island and back," by Mr. W. L. Carpenter; January 28, "Musical Sounds and how we hear them," by Dr. F. W. Mott.

A SECOND edition of Sir William Aitken's "Animal Alkaloids" (H. K. Lewis) has been published. The work has been carefully revised, and the author's aim has been to bring the book up to the present state of knowledge regarding the important subject to which it relates.

THE first part of a monograph of Oriental *Cicadidæ*, by W. L. Distant, has been published by order of the Trustees of the Indian Museum, Calcutta. It is printed in clear type, and includes two fine plates. The monograph, when completed, will evidently be of much scientific value.

M. VAYSSIÈRE has now completed the publication of his "Atlas d'Anatomie Comparée des Invertébrés." It comprises sixty plates, with corresponding letterpress, and is much appreciated by French zoologists.

THE Proceedings and Transactions of the International Agricultural Congress held in Paris last summer have just been issued.

A REUTER'S telegram from Madrid says that a shock of earthquake was felt at Granada on the evening of December 16. There was great alarm for the moment, and the people rushed in panic out of the theatre, where a performance was going on at the time. Apparently no damage was done.

THE Pilot Chart of the North Atlantic Ocean for December states that stormy weather has been prevalent during the month of November. Two notable cyclones have occurred; the first moved eastward from Chesapeake Bay on the night of the 9th. On the 11th it was central in about latitude 41° N., longitude 57° W.; and from this position it moved nearly due north-east, and rapidly increased in energy. The other cyclone moved eastward from the New Jersey coast on the 13th, and was central on the 14th in latitude 42° 40′ N., longitude 63° 20′ W. This storm attained great violence during the 14th and 15th. After the 16th, gales of varying force occurred along the coast north of Florida. There was very little fog during the month; a dense bank was reported on the 17th on the north coast of Cuba. A number of icebergs are still reported in the vicinity of Belle Isle, and several smaller bergs have been seen over the Newfoundland Banks.

AT the meeting of the French Meteorological Society on November 5, M. Teisserenc de Bort gave an account of his researches on barometric gradients. He distinguished two kinds of gradients, one due to the differences of temperature, and another due to the earth's rotation, and pointed out that these two gradients are always superposed, and that their distinction was a matter of importance, for if the first case predominates (a gradient due to difference of temperature), the wind force may increase and the depression become deeper, while in the second case the depression tends to disappear. He thought it was not impossible to make this distinction, for if we know the force of the wind we might calculate the moment of inertia and the corresponding gradient. He also presented a work on the distribution of atmospheric pressure over the surface of the globe. He showed that the distribution of pressure over different meridians varies upwards of an inch on the same parallel according to the season. With the view of finding out the arrangement of the isobars in higher regions of the atmosphere, the author has calculated the pressures by formulæ at various heights, from the pressure and temperature observed at the earth's surface, and compared their accuracy by the readings at some mountain stations, and he has found that most of the irregularities in the distribution of the isobars tend to disappear as we reach the higher regions of the air, and to be replaced by inflexions in the opposite sense. A summary of this paper will be found in the *Comptes rendus* of the French Academy for December 2.

AT a meeting of the Linnean Society of New South Wales on October 30, Mr. A. Sidney Olliff called attention to the extraordinary abundance of a large Noctuid moth—apparently *Agrotis spina*, Gu. (*A. vastator*, Sc.)—during the early part of October in various parts of the country, especially in the vicinity of Sydney, where it appeared in such vast numbers as to cause great consternation amongst those who were not aware that its food in the larval state is confined to low-growing herbage, and that at no stage of its existence does it eat cloth, furs, or feathers. A similar visitation of these moths occurred in October 1867. Mr. Olliff said that *Agrotis spina* was found in great numbers on the summit of Mount Kosciusko and other high points in the Australian Alps, and added that he was of opinion, after extended inquiry, that this species, and no other, was the true Bugong moth, which formerly formed an important article of food amongst the blacks of the Upper Tumut district.

MR. THOMAS CORNISH, Penzance, recently recorded in *Th Zoologist* the occurrence of the "Old English" or "Black" Rat, captured at a place about five miles north-east of Penzance. In the current number of the same periodical he says that immediately after that capture a perfectly trustworthy observer saw near Cambourne, at a place ten miles south-east from where the first specimen was obtained, a Black Rat, which was certainly not the ordinary Hanoverian Rat; and at a later time Mr. Cornish saw and handled another specimen, captured in Paul Parish, about three miles south-west of Penzance. "These facts," says Mr. Cornish, "apparently point to an incursion of this animal, which is gregarious certainly, and probably a vagrant in herds, but not a migrant."

MR. J. R. DOBBINS, San Gabriel, California, contributes to the new number of *Insect Life* (vol. ii. No. 4) a note on the spread of the Australian ladybird. The note is dated July 2, 1889. At that time the Vedolia had multiplied in numbers, and had spread so rapidly that every one of Mr. Dobbins's 3200 orchard trees was literally swarming with them. All his ornamental trees, shrubs, and vines which had been infested with white scale were practically cleansed by this wonderful parasite. "About one month since," says Mr. Dobbins, "I made a public statement that my orchard would be free from 'Icerya by November 1,' but the work has gone on with such amazing speed and thoroughness, that I am to-day confident that the pest will have been exterminated from my trees by the middle of August. People are coming here daily, and by placing infested branches upon the ground beneath my trees for two hours, can secure

colonies of thousands of the Vedolia, which are there in countless numbers sucking food. Over 50,000 have been taken away to other orchards during the present week, and there are millions still remaining, and I have distributed a total of 63,000 since June 1. I have a list of 130 names of persons who have taken the colonies, and as they have been placed in orchards extending from South Pasadena to Azusa, over a belt of country ten miles long and six or seven in width, I feel positive, from my own experience, that the entire valley will be practically free from Icerya before the advent of the new year."

COCOA-NUT butter is now being made at Mannheim, and, according to the American Consul there, the demand for it is steadily increasing. The method of manufacture was discovered by Dr. Schlunk, a practical chemist at Ludwigshafen. Liebig and Fresenius knew the value of cocoa-nut oil or fat, but did not succeed in producing it as a substitute for butter. The new butter is of a clear whitish colour, melts at from 26° to 28° C., and contains 0·0008 per cent. water, 0·006 per cent. mineral stuffs, and 99·9932 per cent. fat. At present it is chiefly used in hospitals and other State institutions, but it is also rapidly finding its way into houses or homes where people are too poor to buy butter. The working classes are taking to it instead of the oleomargarines against which so much has been said during the last two or three years.

A POINT of great importance for the progress of Western science in the Chinese Empire is whether it should be taught in the Chinese or in a foreign language. The subject has been frequently discussed, and quite recently the opinions of a large number of men most prominently engaged in the education of Chinese were collected and published in a Shanghai magazine, the *Chinese Recorder*. The editor says that nine-tenths of these authorities are of opinion that the Chinese language is sufficient for all purposes in teaching Western science. One gentleman states that Chinese students can only be taught science in their own language, and that the long time necessary for them to acquire English for this purpose is wasted; another says that "science must be planted in the Chinese language in order to its permanent growth and development"; a third sees no reason why the vernacular should not be enough to allow the Chinese student to attain the very highest proficiency in Western science, although he admits that there is at present a want of teachers and text-books. Prof. Oliver, of the Imperial University at Pekin, says he has never found English necessary, but has always taught in Chinese. Prof. Russell, of the same institution, finds Chinese sufficient for popular astronomy. On the other hand, Mr. Tenney says that it can only be for the most popular views of science that the vernacular is sufficient. "It is impossible," he says, "for scholars who are ignorant of any European language to attain any such excellence in modern sciences as to enable them to bear comparison with the finished mathematical and scientific scholars of Europe and America." Thus, he continues, as a medium of thought, any Western language is incomparably superior to Chinese in precision and clearness; the student acquainted with a foreign language has a vast field of collateral thought open to him which does not and never will exist in Chinese, and he can keep abreast of the times, which the Chinese student who must depend on translations cannot do. The relation of the Chinese student "to the world of thought is analogous to that of a blind and deaf person in the West, whose only sources of knowledge are the few and slowly increasing volumes of raised type letters which make up the libraries of the blind." As has been said, however, the weight of opinion is against Mr. Tenney.

IN a recent number of *Humboldt*, Herr Fischer-Sigwart describes the ways of a snake, *Tropidonotus tessellatus*, which he kept in his terrarium in Zurich. It was fond of basking in the sun on the top of a laurel, from which it climbed easily to a high cherry-tree fixed against a wall, its night quarters. Sometimes, after lying still for hours, it would hasten down into a small pond (about 4 square yards surface) containing gold-fish, and hide itself for a long time, quite under water, behind some stone, or plants, the tongue constantly playing. When a fish came near, the snake would make a dart at its belly. Often missing, it would lose patience, and swim after the fishes, driving them into some corner, where it at length seized one in the middle of the belly, and carried it to land, much as a dog would a piece of wood. Curiously, the fish, after being seized, became quite still and stiff, as if dead. If one then liberated it, the skin of the belly was seen to be quite uninjured, and the fish readily swam away in the water. The author thinks the snake has a hypnotic influence on its prey (and he had observed similar effects with a ringed snake). It would otherwise be very difficult for the snake to retain hold of a wriggling fish. The snake usually carried off the fish some distance to a safe corner, to devour it in peace.

A SPLENDID find of minerals containing the rare metals of the yttrium and thorium groups has been made in Llano County, Texas (*Amer. Journ. of Science*, December 1889). The whole district for many miles round consists almost entirely of Archæan rocks, granite being met with everywhere, and forming the common wayside rock. Throughout the granite are dispersed veins of quartz, and it is in these veins, and especially the swellings of the veins, that large masses of rare minerals have been found. The largest of these deposits consist of gadolinite and fergusonite, and of two entirely new minerals, to which the names yttrialite and thoro-gummite have been given. The first discovery of gadolinite in Texas was made in 1886, when a pocket of huge crystals and masses aggregating to about 500 kilogrammes was unearthed. Since that time a more complete prospection of the district has revealed the existence of still larger quantities. The gadolinite is generally found in small lumps weighing about half a pound, but frequently also in much heavier masses, and sometimes in immense crystals. One double crystal was found weighing 42 pounds, and a still larger single crystal weighed no less than 60 pounds. And these immense crystals actually contain over 50 per cent. of oxides of the yttrium metals, as do also the massive varieties. The crust of the gadolinite crystals, which appear to be of monoclinic habit, was generally altered into a brownish-red hydrate of waxy lustre; but occasionally, as in case of two particular specimens, the crystals were found in a state of rare beauty and perfection. The new mineral yttrialite, a thorium-yttrium silicate, was discovered associated with and often upon the gadolinite. It was generally altered at the surface to an orange-yellow hydrate of quite different structure to that of the hydrate of gadolinite. One mass of this incrustation was found to weigh over 10 pounds. It contains 46 per cent. of oxides of the yttrium metals. Fergusonite, hitherto an exceedingly rare mineral, occurs in large quantities in the Llano County district, generally in the form of broken interlacing prisms several inches long. Two varieties of it have been identified—one a monohydrated and the other a trihydrated variety. The monohydrated kind forms tetragonal prisms with acute pyramidal terminations, of dull gray exterior, but possessing a brilliant bronze-like fracture. It contains 42 per cent. of yttrium earths and 46 per cent. of columbic acid, Cb_2O_5. The trihydrated variety is similar, but of a dark brown colour. Associated with the fergusonite is the new mineral thoro-gummite, a hydrated uranium thoro-silicate. This mineral is frequently found in well-developed crystals resembling, and having angles very nearly the same as, those of zircon. It contains 22 per cent. of UO_3, 41 per cent. of ThO_2, and 6 per cent. of yttrium earths. Its probable essential composition is $UO_3 . 3ThO_2 . 3SiO_2 . 6H_2O$. Besides these four minerals of special interest to chemists, many more—such as

cyrtolite, molybdite, allanite, tengerite, and a new hydrated thorium-yttrium-lead uranate, termed nivenite—have been found. Altogether, this is the richest find of rare earths which has been heard of for some time, and will probably exert a fresh impetus upon the attempts to set our knowledge of the rare-earth elements upon a surer foundation.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Coati (*Nasua rufa* ♂) from South America, presented by Mrs. Petre; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. S. Stutterd; a Short-eared Owl (*Asio brachyotus*) from Hampshire, presented by Mr. E. Hart, F.Z.S.; two Owen's Apteryx (*Apteryx oweni*) from New Zealand, presented by Captain C. A. Findlay, R.N.R., R.M.S.S. *Ruapehu*; four Common Vipers (*Vipera berus*) from Hampshire, presented by Mr. W. H. B. Pain; a Marsh Ichneumon (*Herpestes galera*) from South Africa, purchased; a —— Troupial (*Xanthosomus frontalis*) from Brazil, received in exchange.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m., December 19 = 3h. 54m. 45s.

Name.	Mag	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 826	—	—	4 9 15	−13 1
(2) γ Eridani	3	Yellowish-red.	3 52 53	−13 46
(3) ε Persei	3	Yellowish-white.	3 50 30	+39 39
(4) δ Persei	3	Bluish-white.	3 51 6	+47 26
(5) 43 Schj.	8	Very red.	4 44 37	+28 20
(6) S Tauri	Var.	—	4 23 10	+ 9 42

Remarks.

(1) This is described in the General Catalogue as "a globular cluster, very bright, small, round, very suddenly brighter in the middle, barely resolvable (mottled as if with stars)." In 1864 Dr. Huggins observed the spectrum, and noted that it was apparently continuous, extending from the orange to the blue, without any traces of either bright or dark lines. It was again observed by Winlock at Harvard College in December 1868, and, strange to say, a bright line spectrum was recorded. "Two distinct bright lines, near each other, and coincident with air-lines λ 5020 ± and λ 4990; a third faint line λ 4900 ± " ("Harvard College Observations," vol. xiii. Part 1, p. 64). These lines were in all probability the three ordinary nebula lines near λ 500, 495, and 486. Winlock describes the nebula as planetary, and gives exactly the same co-ordinates as those given by Huggins and in the General Catalogue. If both observers really saw the same nebula, the results are highly suggestive of variability; but even then there is the difficulty of the recorded resolvability. It is quite possible that, in the four years which elapsed between the observations, the spectrum changed from an apparently continuous one to a discontinuous spectrum, by some action similar to that producing variability in such stars as Mira, but at the same time a change of brightness would also be expected, and of this there is no record. In any case, the nebula is well worthy of further examination.

(2) This star of Group II. is interesting, as being a connecting-link between stars like α Herculis, in which the bands are very wide and dark, and those like Aldebaran, in which there is a line spectrum with only the remnants of the bands in the red. Dunér states that the bands 2–8 are visible, but all of them are narrow and pale. *b*, and presumably D, are very strong. Further observations, with special reference to the lines of hydrogen, are suggested.

(3) A star, hitherto described as of the solar type, of which the usual observations are required. If the star appears to be of the same type of the sun or Capella, special attention should be directed to the presence or absence of dark carbon flutings. It is highly probable that stars like the sun, in which there is a photographic indication of carbon absorption, will subsequently cool down and become stars of Group VI., in which carbon absorption is predominant. If this be the case, all the intermediate stages of mixed metallic lines and dark carbon flutings should be represented amongst the stars.

(4) A star of Group IV., of which the usual observations are required.

(5) This is a star of Group VI. The three ordinary bands of carbon are visible, band 6, near λ 564, being rather pale. A study of Dunér's catalogue of the stars of this group shows that some of those in which band 6 is pale give secondary bands, whilst others do not. This appears to be mainly, though not entirely, due to differences of magnitude. Comparative observations with the same telescope and spectroscope, with reference to this point, are suggested.

(6) Gore states the period of this variable as 378 days, and the magnitudes at maximum and minimum as 9·9 and < 13 respectively. The colour is described as trifling, but the spectrum has not yet been recorded. The maximum will occur on December 28.

A. FOWLER.

PERIOD OF U CORONÆ.—Mr. S. C. Chandler (*Astronomical Journal*, No. 205), from the observations of the period of this star, finds an inequality of the same order as those detected in U Ophiuchi and U Cephei, variables of the Algol type. The period appears to be shortening by 0·0036s. from minimum to minimum. The results depend upon forty-five very unequally distributed minima; thirty-eight, however, lie in the interval 1870–74, and afford a basis to work upon. A larger series of observations is required to elucidate Mr. Chandler's hypothesis, which, however, is quite conformable within the limits of the purely accidental errors of the observations that have been investigated.

IDENTITY OF BROOKS'S COMET (*d* 1889) WITH LEXELL'S COMET 1770.—In the same publication as the above, Mr. Chandler gives some most interesting results of an investigation into the orbits of these comets. The following is a summary of the principal conclusions:—

(1) The encounter of the comet with Jupiter in 1886 effected a complete transformation of the comet's orbit. Instead of the present seven years' ellipse, it was previously moving in a large one of twenty-seven years' period.

(2) Several months before reaching its perihelion, it passed, near the beginning of 1886, into the sphere of Jupiter's attraction, and was deflected into a hyperbolic path about that planet, and narrowly escaped being drawn into a closed orbit, as a satellite of Jupiter.

(3) At the point of closest approach to Jupiter, May 20, 1886, the comet was distant from it only about nine diameters of the planet, passing a little outside of the orbit of the third satellite.

(4) In 1779, and not before, the comet must have come so near to Jupiter as to pass under his control and experience a radical change of orbit at the point of longitude where Lexell's comet underwent its notable disturbance in that year. Moreover, the elements of Lexell's comet before the disturbance were strikingly similar to those found for the present comet previous to 1886.

Taking all the points presented into consideration, the argument for the identity of the two comets is overwhelming. A fuller investigation will be made as soon as the observations for the whole apparition have been received.

SOME PHOTOGRAPHIC STAR SPECTRA.—An examination has been made by Dr. Scheiner of the star spectra photographed at Potsdam (*Astr. Nachr.*, No. 2923). The wave-lengths of lines in the spectra were determined by comparison with the solar spectrum, and as the probable error of the measures is estimated so small as 0·005, the identification of the lines seems beyond doubt. The following are some descriptive results:—

γ Cassiopeiæ. Continuous spectrum; hydrogen lines and D_3 bright.

α Coronæ. The magnesium line at 448·2 appears as a broad line in this star.

α Lyræ. Some fine lines, supposed to be due to iron or calcium, are seen, but have not been measured.

Sirius. 91 similar fine lines to those in the above star have been measured, and 43 ascribed to iron. Even more of these lines occur in Procyon.

α Aquilæ. The spectrum of this star appears almost identical with that of the sun.

β Orionis. The hydrogen and other lines appear broad, but are not diffused at the edges as in α Lyræ and similar stars. 20 lines have been measured from λ 400 to λ 460.

a Aurigæ. 291 lines have been measured in the spectrum of this star between λ 410 and λ 470, all of which appear identical with solar lines.

MAGNITUDE AND COLOUR OF η ARGÛS.—Observations of this variable have been made at Cordoba since 1871, and some comparisons made by Mr. Thome (*Astr. Nachr.*, No. 2922) show that it steadily decreased in magnitude until about the end of 1886, when a minimum of 7·65 was reached, and it is now about 6·6. In 1843, Maclear gave the brightness of η Argûs as − 1·0, or between that of Sirius and Canopus, so that the variation in magnitude is 8·5, and not 6 as heretofore assumed, this variation, extending over 44 years, gives an average yearly rate of diminution of 0·2.

It is interesting to note that the change in magnitude was accompanied by a change in colour; for although before minimum the star was of a dull scarlet the colour became lighter, until in June 1889 it was a bright orange.

ORBIT OF BARNARD'S COMET 1884 II.—From an investigation of all the available observations of this periodic comet, Dr. Berberich has computed the following elements (*Astr. Nachr.*, 2938–39).

Epoch 1884 August 16·5, Berlin Mean Time.

$$
\begin{aligned}
M &= 359^\circ\ 59'\ 49''\!\cdot\!13 \\
\omega &= 301\ \ 1\ 58\cdot63 \\
\Omega &= 5\ \ 8\ 59\cdot12 \\
\iota &= 5\ 27\ 38\cdot40 \\
\phi &= 35\ 44\ 50\cdot92 \\
\mu &= 657''\cdot0839 \pm 0''\cdot8876 \\
\log a &= 0\cdot4882572
\end{aligned}
$$

Perihelion passage = 1884 August 16·516543
Period = 1972·35 ± 2·66 days.

It will be seen from the foregoing period, that the comet will be at perihelion again in 1890 January 9·87.

ALGOL.—At the meeting of the Royal Prussian Academy of Sciences, held on November 28, Prof. Vogel gave the results he had obtained from photographs of the spectrum of this variable. Prof. Pickering had pointed out, some years ago, that if the variation in stars of the Algol class were due to the transit of a dark satellite across the disk of its primary, producing a partial eclipse, then since in every case yet known the two bodies must be close to each other, and of not very disproportionate size, the primary must revolve with very considerable rapidity in an orbit round the common centre of gravity of the two; and, therefore, be sometimes approaching the earth with great rapidity and sometimes receding from it. Six photographs of the spectrum of Algol—obtained, three during last winter, and three during the November just past—have shown that before the minimum the lines of the spectrum of Algol are markedly displaced towards the red, showing a motion of recession; but that after the minimum the displacement is towards the blue, showing a motion of approach. Assuming a circular orbit for the star, and combining the details given by the spectroscope with the known variation of the star's light, Prof. Vogel derives the following elements for the system of Algol:—

Diameter of Algol...	1,074,100 English miles.
Diameter of the dark companion	840,600 ,, ,,
Distance of centre...	3,269,000 ,, ,,
Speed of Algol in its orbit	27 miles per second.
Speed of the companion in its orbit ...	56 ,, ,,
Mass of Algol	$\frac{4}{9}$ of the sun.
Mass of the companion	$\frac{2}{9}$,, ,,
Speed of translation of the entire system towards the earth	2 miles per second.

It will be seen that the density both of Algol and its companion is much less than that of the sun—less than a quarter, in fact. This is what we might expect, for Algol and all the variables of its class yet examined give spectra of Group IV., and should therefore represent a less advanced stage of condensation than that seen in our sun. This demonstration of the truth of the satellite theory of variation of the Algol type derives also an especial interest from Prof. Darwin's researches on tidal evolution, for assuming, as we well may, that the cause of variation is the same in all members of the class, we now know of nine stars in which a large companion is revolving round its primary at but a very short distance from it, and in a very short space of time. The companion of U Ophiuchi must, indeed, be almost in contact with its parent star.

DISCOVERY OF A NEW COMET.—A faint comet was discovered by M. Borrelly at Marseilles, on December 12, at 7h. 49·5m. G.M.T. R.A. 18h. 7m.; daily motion in R.A. + 1m. 12s. N.P.D. 41° 7′; daily motion + 60′.

GEOGRAPHICAL NOTES.

WE regret to have to record the death of Major Peter Egerton Warburton, whose name will always be intimately associated with the history of exploration in Australia. He died at Beaumont, Adelaide, in his seventy-sixth year. His most famous achievement, undertaken in 1873, was the crossing of the continent from a point on the overland telegraphic line to the De Grey River, in Western Australia. Nothing was heard of him for about twelve months, during which he and his party suffered terrible privations in their march across the desert. After the expedition, Major Warburton visited England, and was awarded a Gold Medal of the Royal Geographical Society for his efforts towards increasing our knowledge of the interior of Australia. He received the Companionship of the Order of St. Michael and St. George in 1875.

THE death is announced of Cardinal G. Massaja in his eighty-first year, at St. Georgio a Cremano. For nearly half a century the name of this distinguished explorer has been intimately associated with the progress of geographical discoveries in Abyssinia and the surrounding regions. It was at his suggestion that the Italian Geographical Society organized the Antinori Expedition to Shoa, which has resulted in the occupation of a vast region, and the extension of Italian influence over the whole of Ethiopia. His chief work, "I miei trentacinque Anni nell' alta Etiopia," abounds in valuable geographical, historical, and ethnological information on the East African regions for so many years explored and studied by him. The Cardinal was born at Piovà in 1809, and, in 1846, appointed Vicar Apostolic of the Galla nation.

FROM the Berlin Correspondent of the *Daily News* we learn that a full account of the ascent of Kilimanjaro by Dr. Hans Meyer and Prof. Purtscheller has been received at Berlin. It is dated "Marangu Jagga, October 9." The journey from Zanzibar to Uawela took exactly a fortnight. On September 25 the travellers reached Marangu. On October 2 they encamped, with a Pangani negro, on the ridge of the plateau, at a height of 14,450 feet. At 2.30 a.m. they started for the lava-ribs surrounding the valley of glaciers to the south about 1200 feet above. At 7 o'clock, on the right side of the valley, at an elevation of about 16,500 feet, the first snow was seen under cover of the rocks. The higher they went, the more clefts and fissures the field of ice had. The travellers say:—"After great exertions we reached, at 1.45, the snow-line, and it was seen that the highest peak, which was formed of rocks jutting out of the snow, was about one and a half hour's march to the left. After resting a day and a half we set off, on October 5, to bivouac in the Lava Cave, at a height of about 15,200 feet, and on the next day we repeated the ascent. The peaks were gained without particular difficulty, and on the central and highest one, 19,680 feet above the sea, the German flag was planted." Dr. Meyer proposes to call this peak Kaiser Wilhelm Peak. The view from here on to the Kibbs Crater—which is 6600 feet broad and 660 feet high, and the lower half of which is encased in a mighty belt of ice, whilst a volcanic cone of about 500 feet rises in the centre—is magnificent. The beauties of the landscape in the Kilimanjaro region seem to be quite extraordinary. On October 10 the Kimawensi was to be ascended. The two travellers enjoy the best of health.

THE double number of the *Bollettino* of the Italian Geographical Society for October and November, which appears some weeks behind time, is largely devoted to African subjects, and more particularly to the north-eastern region, which is rapidly becoming an "Italian colony." Captain D. Stasio publishes a summary of Don Francesco Alvarez's "Travels in Ethiopia" in the sixteenth century, enriched with valuable notes and additions. Alvarez, a priest attached to an embassy forwarded by Portugal, in 1520, to the Emperor of Abyssinia, shows himself a careful observer of men and things, and his work, which was included in Ramusio's "Navigationi et Viaggi" (Venice, 1588), abounds in details regarding the political, social, and economic relations of that region in the sixteenth century. Giulio D. Cocorda brings to a conclusion his important series of papers

on the South African gold-fields, which include much information on the present condition of the whole of South Africa as far north as the Zambesi. The observer points out that, while the Delagoa Bay and other lines of communication are much discussed, the fine artery of the perfectly navigable Limpopo is entirely neglected, notwithstanding Captain Chaddock's navigation of it a few years ago. The writer remarks that "this river flows mainly through regions under the influence or protectorate of England; the Transvaal people on the one side, and those of Matabeleland on the other, would certainly be glad to avail themselves of this outlet for their produce. As it traverses only a small tract of Portuguese territory about its estuary, I hope and believe that Portugal will not be allowed to treat the Limpopo as she is now attempting to treat the Zambesi. The subject is of such importance that it cannot fail soon to be brought before the British Parliament." Referring to the negotiations at present going on in connection with the Swaziland question, he observes, in the same spirit:—"The Swazi people must, sooner or later, yield either to the Transvaal or to England, and if to the former, it must be to the entire detriment of British interests. England, as the suzerain power in South Africa, should be the first in the field, both in her own interest and in that of her other colonies and subjects. If she does not assume the protectorate of Swaziland, besides losing the control of a vast and rich mineral district, she will deprive the colony of Natal of all further hope of expansion. If she ignores her responsibility in this matter, and allows the Transvaal Republic to absorb Swaziland, she will add another to the long list of blunders that threaten to destroy all prospect of consolidating a dominion as large as Canada, and may end disastrously for British interests in South Africa."

A FRENCH traveller has just achieved a feat of great interest. Captain Trivier, equipped by the newspaper *La Gironde*, started some eighteen months ago for the Congo State. He went up the river to Stanley Falls, and thence proceeded to Central Africa and the Lake region, accompanying caravans. He has just arrived at Mozambique.

Globus reports that during the past summer M. Thoroddsen, the well known student of Iceland, has carried out a journey in the waste region known as Fiskivötn, lying between Hecla and the Vatna Jökul, which has hitherto been unvisited for the most part by any inquirer. To the east and north of Hecla he discovered a new obsidian region. Crossing the Tunguaa, he went to the Fiskivötn group of lakes, all true crater lakes. The district between this and the Vatna Jökul has absolutely no plant-life whatever; it consists of lava-fields, and plains of volcanic sand. In it he found a lake, Thorisvatn, the second largest in the island. Thence, after a day's journey through an utterly desolate district, he reached the hitherto unknown source of the Tunguaa. To the south of this he discovered, between three ranges of hills, previously unknown, a new and very long lake.

MR. DAUVERGNE has, says the *Times of India*, completed an adventurous journey in the regions of North-West Cashmere. His course was from Leh northwards to the Kilian Pass, in Kashgaria, and then northwards across the Pamir to the Upper Oxus. He reached Sarhad in safety, and after six days' halt there, crossed the Hindu Kush by the Baroghil Pass, as he did not wish to visit Chitral. He then turned eastwards, and after a trying journey through the snow, crossed the Ishkaman Pass, north of Yasin. Thence he travelled southwards by the Karambar Valley, and eventually reached Gilgit, a short time after Captain Durand had started for Chitral. Mr. Dauvergne reports that the Russian explorer, Captain Grombchevsky, whose attempt to reach Kafiristan was noticed some time ago, was stopped at Kila Panjah on the Oxus, by the Afghan authorities.

THE ST. PETERSBURG PROBLEM.

THIS celebrated problem, which is first mentioned before 1708 in a letter from the younger Nicholas Bernoulli to Montmort, has been frequently discussed by Daniel Bernoulli (1730) and other eminent mathematicians. It may be briefly stated as follows:—

A tosses a coin, and undertakes to pay B a florin if head comes up at the first throw, two florins if it comes up at the second, four florins if it be deferred until the third throw, and so on. What is the value of B's expectation?

The chance of head appearing at the
1st, 2nd, 3rd, 4th . . . . nth throw is
$\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$ . . . . $\frac{1}{2}^n$. A promises to pay for head
1, 2, 4, 8 . . . . 2^{n-1} florins, hence B's expectation is
$\frac{1}{2}$, $\frac{2}{4}$, $\frac{4}{8}$, $\frac{8}{16}$ . . . . $2^{n-1}/2^n = \frac{1}{2}$ florin.

Hence the total value of B's expectation is an *infinite* series, each term of which is a shilling, or it is infinite.

This result of the theory of probability is apparently directly opposed to the dictates of common-sense, since it is supposed that no one would give even a large finite sum, such as £50, for the prospect above defined.

Almost all mathematical writers on probability have allowed the force of the objection, which they have endeavoured to evade by various ingenious artifices all more or less unsatisfactory.

The real difficulty of the problem seems to lie in the exact meaning of *infinite* and *value of the expectation*.

Since the infinite value of the result is *only* true if an infinite number of trials are paid for and made, all such considerations as want of time and the bankruptcy of A or B are precluded by the terms of the question.

The value of B's expectation is frequently confused with how much he can or ought to pay for it; thus Mr. Whitworth ("Choice and Chance," p. 234) finds that if B have 1024 florins, he may give very little more than 6 florins for the venture. This ingenious, solution seems to have no reference to the original problem, which has been modified by Mr. Whitworth's introduction of the word "advantageously" (p. 232).

B can pay for his expectation in three ways: (i.) a sum before each toss; (ii.) a sum before each series of tosses ending with head; (iii.) a sum for the total result of A's operations.

Mr. Whitworth apparently assumes the first method of payment, and shows that the larger B's funds are the more he may safely pay for each toss, since he can continue to play longer. Many mathematicians take the second method of payment. "However large a fee I pay for each of these sets, I shall be sure to make it up in time" ("Logic of Chance," p. 155).

It is easy to show in this case also that what may be safely paid before each series increases with the number of series.

Suppose a very large number of tosses made, about half would come up heads and half tails; each head would end a series, when a fresh payment must be made by B. Suppose the tosses limited to one series, if B pays one florin he cannot possibly lose, if he pay anything more he may lose by head coming up the first time, and the more he pays the greater will his chance of loss be, since the series of tails must be longer to cover it. But, however large a finite sum he pays, he is not *certain* to lose, *e.g.* head *may* not come up till the hundred and first toss, when he would receive

2^{100} = 1,267 650,600 228,229 401,496 703,205 376 florins.

If the sets are limited to one hundred, about

50	heads would	probably come	up the	1st	toss.
25	,,	,,	,,	2nd	,,
13	,,	,,	,,	3rd	,,
6	,,	,,	,,	4th	,,
3	,,	,,	,,	5th	,,
2	,,	,,	,,	6th	,,
1	,,	,,	,,	7th	,,

B would receive for each series 50 florins.

Hence for the hundred sets, B would receive about 350 florins, or he could pay without loss seven shillings for each set.

If N be the number of sets, the total amount received by B will probably not be less than n terms of the series

$$\left\{\frac{N \times 2^0}{2^1} + \frac{N \times 2^1}{2^2} + \&c.\right\} = n\left\{\tfrac{1}{2}\right\}N,$$

but n is the number of times which N is successively divisible by 2, or $2^n = N$, or $n = \log N/\log 2$. But the amount x which B can afford to pay per set when multiplied by the number of sets is equal to the amount which he receives, or—

$$xN = \frac{\log N}{\log 2}\left\{\tfrac{1}{2}\right\}N,$$

hence $x = \log N/0{\cdot}6$ nearly.

This formula, though inexact for low, is very convenient for high, values of N.

$N = 1$	$x = 0$	$N = 10^6$	$x = 10$
$= 50$	$= 2{\cdot}7$	$= 10^9$	$= 15$
$= 100$	$= 3{\cdot}3$	$= 10^{12}$	$= 20$
$= 1000$	$= 5$	$= 10^{15}$	$= 25$

x increases with, though much more slowly than, N, and becomes infinite when N does. But to justify a payment of

£50 per set, we must expect a number of sets represented by 301 figures.

Lastly, what is the value of B's expectations if A's operations are continued indefinitely. With great deference to contrary opinions, I believe this to be the correct meaning of the problem in its original form. The theoretical result is in this case easily realized by the aid of the following illustration. Suppose the person A replaced by an automatic machine similar to that used for weighing sovereigns, which tosses continuously ten times per minute. On the average of a large number of tosses, B cannot receive less than one shilling a toss, £1 every two minutes, or £720 a day for ever. If the current rate of interest be 3 per cent., he may safely pay for this perpetual annuity £8,760,000. Suppose, instead of this comparatively slow rate, the machine increased the rapidity of its operations indefinitely, the sum to be paid for the result would also increase indefinitely, or the expectation would become infinite.

SYDNEY LUPTON.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Newall Telescope Syndicate has drawn up a scheme for building a dome for the telescope on a site adjoining the present Observatory, with an observer's house; and they recommend that an observer be appointed, at a stipend of £250 per annum, with a house, to devote himself to research in stellar physics, under the general direction of the Director of the Observatory.

The results of this year's commercial examination, held by the School's Examinations Board, are satisfactory. Geography was still very imperfect. Elementary mechanics has now been added to the list of compulsory subjects.

An influential syndicate has been appointed to consider the question of the mechanical workshops, their management and utility.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 12.—"An Experimental Investigation into the Arrangement of the Excitable Fibres of the Internal Capsule of the Bonnet Monkey (*Macacus sinicus*)." By Charles E. Beevor, M.D., F.R.C.P., and Victor Horsley, B.S., F.R.S. (from the Laboratory of the Brown Institution).

After an historical introduction, the authors proceed to describe the method of investigation, which was conducted as follows. The animal being narcotized with ether, the internal capsule was exposed by a horizontal section through the hemisphere. By means of compasses the outline of the basal ganglia and capsule were accurately transferred to paper ruled with squares of one millimetre side, so that a projection of the capsule was thus obtained, divided into bundles of one millimetre square area. Each of these squares of fibres was then excited by a minimal stimulus, the same being an induced or secondary interrupted current. The movements were recorded and the capsule photographed.

In all forty-five experiments were performed, and they are arranged in eight groups, representing eight successive levels (*i.e.* from the centrum ovale to the crus) at which the capsule was investigated.

Before the results are described in detail a full account is given of previous investigations, experimental, clinical, and anatomical, on the arrangement of the internal capsule.

The anatomy of the part and the relation of the fibres to the basal ganglia are then discussed, and a full description given of each of the groups examined.

The general results are next given at length, of which the following is a *résumé*.

Firstly, the rare occurrence of bilateral movement is discussed, and the meaning of the phenomenon defined. Secondly, the lateral arrangement and juxtaposition of the fibres are considered. Thirdly, the antero-posterior order in which the fibres for the movements of the different segments are placed is described, and that order found to be practically identical with that observed on the cortex, viz. from before back:—

Movements of eyes.
" head.
" tongue.
" mouth.
" upper limb (shoulder preceding thumb).
" trunk.
" lower limb (hip preceding toes).

The character or nature of these movements is set out in a table giving the average localization of each segment. Speaking generally, it may be said that the movements are arranged in the same way as has already been shown by the authors to exist in the cortex (*vide* previous papers in Phil. Trans., 1887, 1888), viz. that the representation of extension is situated in front of flexion for the segments of the upper limb, while for the toes flexion is obtained, as in the cortex, in front of extension.

Numerous tables and diagrams are appended, showing the extent of appropriation of fibres for each movement.

Physical Society, November 15.—Prof. Reinold, F.R.S., President, in the chair.—Mr. Enright resumed the reading of his paper on the electrification due to contact of gases with liquids. Repeating his experiments with zinc and hydrochloric acid, the author, by passing the gas into an insulated metallic vessel connected with the electrometer, proved that it was always charged with electricity of the opposite kind to that of the solution. The electrical phenomena of many other reactions have been investigated, with the result that the gas, whether H, CO_2, SO_3, SH_2, or Cl, is always electrified positively when escaping from acids, and negatively when leaving a solution of the salt. In some cases distinct reversal is not obtainable, but all these seem explicable by considering the solubility and power of diffusion of the resulting salts. Various other results given in the paper tend to confirm this hypothesis. Seeking for an explanation of the observed phenomena, the author could arrive at no satisfactory one excepting "contact" between gases and liquids, and if this be the true explanation he hoped to prove it directly by passing hydrogen through acid. In this, however, he was unsuccessful, owing, he believes, to the impossibility of bringing the gas into actual contact with the liquid. True contact only seems possible when the gas is in the nascent state. Some difficulty was experienced in obtaining non-electrified gas, for the charge is retained several hours after its production, even if the gas be kept in metallic vessels connected to earth. Such vessels, when recently filled, form condensers in which the electricity pervades an inclosed space, and whose charge is available on allowing the gas to escape. Soap bubbles blown with newly generated hydrogen were also found to act as condensers, the liquid of which, when broken, exhibited a negative charge. This fact, the author suggested, may explain the so-called "fire-balls," sometimes seen during thunderstorms; for if, by any abnormal distribution of heat, a quantity of electrified air becomes inclosed by a film of moisture, its movements and behaviour would closely resemble those of fire-balls. A similar explanation was proposed for the phenomenon mentioned in a recent number of NATURE, where part of a thundercloud was seen to separate from the mass, descend to the earth, and rise again. The latter part of the paper describes methods of measuring the contact potential differences between gases and liquids, the most satisfactory of which is a "water dropper," and by its means the P.D. between hydrogen and hydrochloric acid was estimated to be about 42 volts. Prof. Rücker asked if the experiment with zinc and hydrochloric acid could be started in the second stage by having the acid partly saturated with salt. Dr. C. V. Burton thought it probable that contact could be made between a gas and a liquid by shaking them up together in a bottle. In reply, Mr. Enright said the experiment could be started at any stage, and reversal effected as often as desired by adding either acid or a solution of salt to the generating vessel.—Mr. Herbert Tomlinson, F.R.S., read a paper on the effect of repeated heating and cooling on the electrical resistance and temperature coefficient of annealed iron. In a paper recently presented to the Royal Society, the author has brought forward an instance of an iron wire, which when subjected to magnetic cycles of minute range alternately at 17° and 100° C., had its molecular friction and magnetic permeability reduced respectively to about one-quarter and one-half their original values. The present experiments were undertaken to see whether by

such heatings and coolings the temperature coefficient of iron could be brought down to something approaching the number given by Matthiessen for "most pure metals." The wire experimented on was first annealed by heating to 1000° C. for several hours and allowing to cool slowly in a furnace placed at right angles to the magnetic meridian; the process was repeated three times. Afterwards the wire was covered with paper and wound doubly into a coil. This coil was inclosed in a water-jacketed air-chamber, and connected with a sensitive Wheatstone bridge. Thermo-electric and Peltier effects were eliminated by always keeping the galvanometer circuit closed. By repeated heating to 100° C. and cooling to 17° C. for long intervals, the specific resistance at 17° C. was reduced from 11,162 to 10,688 C.G.S. units, after which the operations produced no further change. At the same time the temperature coefficient increased in the proportion of 1 : 1·024. From careful determinations of the resistance at different temperatures, the formula $R_t = R_0(1 + 0·005131t + 0·00000815t^2)$ was deduced, whilst that obtained from Matthiessen's results for pure iron annealed in hydrogen is $R_t = R_0(1 + 0·005425t + 0·0000083t^2)$. Taking his own determination of specific resistance of impure iron as correct, coupled with Matthiessen's law connecting the resistances and temperature coefficients of metals and their alloys, the author finds that the specific resistance of pure iron deduced from Matthiessen's results is from 4 to 5 per cent. too high. In conclusion, Mr. Tomlinson expresses a hope that the B.A. Electrical Standards Committee may be induced to determine the absolute resistance and temperature coefficient of the pure metals which are in ordinary use. Prof. Ayrton thought Matthiessen's results were expressed in B.A. units, and hence might appear 1 or 2 per cent. too great. Mr. Tomlinson, however, believed the number he took were expressed in legal ohms. Dr. Walmsley asked for what value of the magnetizing force the permeability of the iron mentioned in the beginning of the paper was determined; to which Mr. Tomlinson replied that they were much smaller than the earth's horizontal component.—Dr. Thompson's paper on geometrical optics was postponed.

EDINBURGH.

Royal Society, December 2.—Sir Douglas Maclagan, Vice-President, in the chair.—Prof. Tait communicated a paper by Dr. G. Plarr, on the transformation of Laplace's coefficients.—Mr. A. C. Mitchell read a preliminary note on the thermal conductivity of aluminium. A comparatively rough first experiment shows that this metal slightly exceeds good copper in conductivity.—Dr. John Murray discussed the question of the origin and nature of coral reefs and other carbonate of lime formations in recent seas. He first referred to experiments which have recently been made regarding secretion and solution of carbonate of lime. Carbonate of lime remains are found in great abundance at the sea bottom in shallow waters, but the amount steadily diminishes as the depth increases, until at 4000 fathoms almost every trace has disappeared. This is due to solution, as the organisms slowly fall to the bottom. Everywhere within 500 fathoms of the surface the ocean teems with life. The Greely Expedition was starving within ten feet of abundant food which might have been obtained by breaking a hole through the ice and using a shirt as a drag-net. Dr. Murray then proceeded to discuss his theory of the formation of coral reefs, bringing forward in reply to objections by Dana and others, some recently obtained facts regarding the existence of shallow regions in what is, on the whole, deep water. He showed that carbonate of lime is continually produced in great quantity in warm tropical water by the action of sulphate of lime in solution on effete products. This explains the great growth of coral in tropical regions. The absence of coral on certain shores in tropical districts is explained by the uprise of cold water due to winds blowing off shore. His paper was illustrated by an elaborate series of lime-light diagrams.

PARIS.

Academy of Sciences, December 9.—M. Hermite in the chair.—On the nitrification of ammonia, by M. Th. Schlœsing. In a recent communication (September 9) the author described three experiments on the nitrification of ammonia in vegetable humus, tending to prove that this phenomenon is accomplished without any appreciable loss of nitrogen liberated in the gaseous state. He now reports the results of two other experiments, showing that this is no longer the case when a larger proportion of ammonium carbonate is introduced into the soil.—Correction in the tables of Jupiter's movement worked out by Le Verrier, by M. A. Gaillot. Comparing the secular terms of the eccentricity and perihelion of Jupiter's and Saturn's orbits as determined by Le Verrier, Hill (*Astronomical Journal*, No. 204) came to the conclusion that there must be an error of sign in the terms of the second order relating to Jupiter's orbit. M. Gaillot has now gone over the calculations again, and finds that Le Verrier's manuscript is correct, but that, as conjectured by Hill, a misprint of a sign occurs in the published work. In vol. x. p. 242, the sign + appears instead of – before the term $0''·015,554\beta' \cos(\omega - \pi')$.—On the characteristic temperatures, pressures, and volumes of bodies, by M. Ladislas Netanson. These researches tend to show that for every gas there exists an infinite number of characteristic values, t, p, v, which, being adopted as units of the general variables t, p, v, have the remarkable property of eliminating all difference in the characteristic equations of the different gases. The systems usually employed in measuring temperatures, pressures, and volumes, having nothing in common with the intimate nature of the bodies themselves, give rise to differences in the equation $F(t, p, v) = 0$, which disappear when for each body the physicist employs a special system in accordance with its properties.—On the localization of the interference fringes in thin isotropic plates, by M. J. Macé de Lépinay. In studying the exact conditions of the fringes in thin prismatic plates, the author finds a complete verification of the general theory expounded by him in a previous communication (*Comptes rendus*, July 22, 1889). The consequences of the theory may be considered as entirely verified by these experiments.—On the want of accuracy in thermometers, by M. E. Renou. On a recent occasion (July 1) M. Cornu remarked that hitherto these instruments have been liable to an error of from 0°·2 to 0°·3. It is now shown that observations hitherto recorded may give rise to the greatest inconvenience, more perhaps in future than at present. These remarks were supplemented by M. Cornu, who pointed out that errors in the mercury thermometer as great as 0°·2 or 0°·3 occur only in observations taken at considerable intervals of temperature and with instruments not sufficiently tested.—Variations in the mean temperature of the air at Paris, by M. Renou. Twenty years ago the author attempted to show that severe winters return in groups of five or six every forty-one years. This somewhat elastic period is perhaps reproduced better in groups of years than in single years. It also appears that the Observatory of Paris gives a mean temperature higher by 0°·7 than that of the surrounding rural districts—10°·7 as compared with 10°·0 of the Parc Saint-Maur Observatory.—On the observations of temperature on the top of the Eiffel Tower, by M. Alfred Angot. These observations, begun on July 1, are being still continued with a Richard registering thermometer, placed 336 metres above the sea, and about 301 above the ground. Compared with those of the Parc Saint-Maur (50 metres) they show that the normal decrease of about 1° for every 180 metres is greatly exceeded in summer and during the day (means of the maxima), and correspondingly diminished in winter and at night (means of the minima); or there is generally even an inversion in the temperatures, the air being then warmer at 300 metres than near the ground.—Papers were submitted by M. Raoul Varet, on the ammoniacal cyanides of mercury; by M. L. Prunier, on the simultaneous quantitative analysis of sulphur and carbon in substances containing sulphur; by M. E. Guinochet, on an acid isomerous with tricarballylic acid; by M. C. Tanret, on two new sugars extracted from quebracho (*Aspidosperma quebracho*); by M. Arnand, on carotine, its probable physiological action on the leaf; and by MM. André Thil and Thouroude, on a micrographic study of the woody tissues of native trees and shrubs, prepared for the special exhibition of the Forest Department.—The sealed paper, by M. A. Joannis, on compounds of potassium and sodium with ammonia gas, was opened by the Secretary.

BERLIN.

Physical Society, November 22.—Prof. du Bois Reymond, President, in the chair.—Dr. Lehmann spoke on the nature and distribution of the Babylonian metrical system. He expressed his desire to lay before the competent judgment of the Physical Society, the results of his most recent archæological researches, so far as they are of direct physical interest, and then proceeded to describe the numerical system employed by the ancient Baylonians, explaining that it consisted of a sexagesimal system with decimal subdivisions. The unit of time, the double-

minute, was the time occupied by the sun's rising, measured at the Equinox, and could thus be recovered at any time. It was measured by the mass of water which flowed out of a certain vessel from the instant at which the upper edge of the sun appeared above the horizon to the moment at which his lower edge was exactly touching the horizon. The day consisted of 720 of these units. The unit of length was the ell, which was used in two forms, either as a single- or double-ell; subdivisions used were the foot = $\frac{1}{3}$ double-ell, the hand-width, and the finger-length. The unit of weight was the mine, also occurring as single-mine or double-mine. The derivation of units of weight from units of length, as in the modern case of grams and centimetres, was also known, but of course the water used was not distilled and was not weighed at 4° C. The speaker had, however, succeeded in discovering a measuring-scale on an ancient monument dating from the year 2500 B.C., which had enabled him to compare the Babylonian measures with those of our own time. It appeared from this that a hand-breadth = 99·4–99·6 mm.; a double-ell = 994–996 mm.; and the foot = 331–332 mm. He had further discovered several stamped weights, and thus found that the double-mine = 982·4–985·8 grams. The single-mine weighed half as much as the double-mine, but the gold-mine and silver-mine were equal to five-sixths of a single-mine. The royal-mine was 1 per cent. heavier than the gold-mine, and was employed for the payment of tribute. The coinage was based upon the mine and its sexagesimal division. Dr. Lehmann remarked how surprising it is to find that the length of a seconds-pendulum at Babylon is 992·5 mm., and was inclined to advance the hypothesis that the Babylonian unit of length was derived from a seconds-pendulum, the foot being one-third the lengh of the pendulum. He next proceeded to give an account of the spread of the Babylonian mine, and of the Phœnician which was derived from it, as a unit of weight among the civilized nations of Europe. The discovery of an old Roman balance had enabled him to determine that the old Etrurian pound was equal in weight to the Babylonian mine. The Babylonian unit of weight is found not only in Italy and the Mediterranean generally, but also the old Dutch and French pounds and the Russian pood are equal in weight to the mine. The speaker considered it to be quite impossible that in all the above cases we are dealing with a chance correspondence between the several weights. In the discussion which ensued, objections were raised on several sides against the hypothesis that the ancient Babylonians had knowledge of the seconds-pendulum, which had subsequently been lost. On the other hand, it was pointed out by others that the ancients were not improbably acquainted with the plummet, and used it for squaring stones, &c.; and since, further, they employed the double-minute as unit of time, it is not impossible that they were acquainted with the seconds-pendulum. The cause of their not having employed this instrument to supply a time-unit may perhaps be found in their ignorance of any means by which the pendulum could be kept in continuous and uniform motion. In conclusion, the speaker laid stress on the high state of culture which the Babylonians had attained three thousand years B.C., and expressed his regret that a complete blank exists with regard to everything of an earlier date than the cuneiform inscriptions.

STOCKHOLM.

Royal Academy of Sciences, November 13.—On the vegetation of the southmost part of the Isle of Gotland, by Prof. Wittrock.—Myxochæte, a new genus of fresh-water Algæ, by Herr K. Bohlin.—On determinations of the longitude and observations on the pendulum executed in Sweden during the year 1889, by Prof. Rosén.—On a reform in the analysis of gaseous bodies, by Prof. O. Pettersson.—On the invariants of linear, homogeneous differential equations, by Prof. Mittag-Leffler.—The form of the observations on linear differential equations, by Herr A. M. Johanson.—On the case of Poincaré as to the three bodies problem and some analogous dynamical propositions, by Herr E. Phragmén.—On the observations made at the Observatory of Upsala for the determination of the equinoctium in the spring of 1889, by Dr. K. Bohlin and Herr C. A. Schultz-Steinheil.—Definitive orbit elements of the comet 1840 iv., by Herr Schultz-Steinheil.—Study of the infra-red spectra of carbonic acid and of carbonic oxide, by Dr. K. Ångström.—On the action of nitric acid on naphthalin-α-sulphon acid, by Prof. P. J. Cleve.—On naphthalin-1-5, calogene-sulphon-acids, by Herr R. Mansélius.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, DECEMBER 19.

ROYAL SOCIETY, at 4.30.—(1) Comparison of the Spectra of Nebulæ and Stars of Groups I. and II., with those of Comets and Auroræ; (2) the Presence of Bright Carbon Flutings in the Spectra of Celestial Bodies: Prof. J. N. Lockyer, F.R.S.—Some Observations on the Amount of Luminous and Non-luminous Radiation emitted by a Gas-flame: Sir J. Conroy, Bart.—On the Effects of Pressure on the Magnetization of Cobalt: C. Chree.—On the Steam Calorimeter: J. Joly.—On the Extension and Flexure of Cylindrical and Spherical Thin Elastic Shells: A. B. Basset, F.R.S.

LINNEAN SOCIETY, at 8.—Intensive Segregation and Divergent Evolution in Land Mollusca of Oahu: Rev. John T. Gulick.—Dictopteris; with Remarks on the Systematic Position of the Dictyotaceæ: T. Johnson.

CHEMICAL SOCIETY, at 8.—On Frangulin: Prof. Thorpe, F.R.S., and H. H. Robinson.—Arabinon, the Saccharon of Arabinose: C. O'Sullivan, F.R.S.—Note on the Identity of Cerebrose and Galactose: H. T. Brown, F.R.S., and Dr. G. H. Morris.

SUNDAY, DECEMBER 22.

SUNDAY LECTURE SOCIETY, at 4.—Algeria and Morocco: some Artistic Experiences (with Oxyhydrogen Lantern Illustrations): Henry Blackburn.

SATURDAY, DECEMBER 28.

ROYAL INSTITUTION, at 3.—Electricity (adapted to a Juvenile Auditory): Prof. A. W. Rücker, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

East Africa and its Big Game: Sir J. C. Willoughby (Longmans).—Measurement of Small Mammals, &c.: Dr. C. H. Merriam (Washington).—North American Fauna, Nos. 1 and 2: Dr. C. H. Merriam (Washington).—Report of the Ornithologist and Mammalogist for 1888; Dr. C. H. Merriam (Washington).—Physical Memoirs, vol. i., Part 2 (Taylor and Francis).—Journal of the Royal Agricultural Society, October (Murray).—Mitteilungen des Vereins für Erdkunde zu Halle A/s, 1889 (Halle).—Proceedings of the Academy of Natural Sciences of Philadelphia, Part 2, 1889 (Philadelphia).—Notes from the Leyden Museum, vol. xi., No. 3 (Leyden, Brill).—

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